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THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND PHARMACY.

VOLUME SEVENTY-SEVENTH.

1852.

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2. Notices of the Effects of Lead upon the System. By James Alderson.

3. On a Remarkable Effect upon the Human Gums, produced by the absorption of Lead. By Henry Burton.

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3. A Short Sketch of the Life and Writings of the late Joseph Clarke.
4. Parturition and the Principles and Practice of Obstetrics. By W. Tyler Smith.
5. On the Theory and Practice of Midwifery. By Fleetwood Churchill.
6. A Theoretical and Practical Treatise on Human Parturition. By H Miller.
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Bradshaw's Companion to the Continent: a descriptive Handbook to the chief places of resort; their characteristic features, climates, scenery, and remedial resources; with observations on the influence of climate and travelling. By Edwin Lee, Honorary and Corresponding Member of the principal Continental Medical Academies and Societies, Author of the Baths of Germany, &c. London, Manchester, 1851. Post 8vo, pp. 408.

Physiological Researches. By Sir Benjamin Brodie Bart., D.C.L.F.R.S.; Corresponding Member of the Academy of Sciences of the Institute of France. Collected and published from the Philosophical Transactions. London, 1851. 8vo, pp. 146.

Report of the Proceedings of the Pathological Society of London. Fifth Session, 1850-51. London, 1851. 8vo, pp. 196. Three Lithograph Engravings.

The Pocket Formulary, and Synopsis of the British and Foreign Pharmacopœias: comprising standard and approved Formulæ for the Preparations and Compounds employed in Medical Practice. By Henry Beasley. Fifth Edition. Corrected, enlarged, and adapted to the last editions of the Pharmacopœias. London, 1851. 12mo, pp. 546.

Guy's Hospital Reports. Second Series. Edited by Edmund Lloyd Birkett, M.D. Cantab.; Fellow of the Royal College of Physicians; Physician to the City of London Hospital for Diseases of the Chest, and Curator of the Museum of Guy's Hospital; and Alfred Poland, Fellow of the Royal College of Surgeons; Assistant-Surgeon to Guy's Hospital, &c. Volume Seventh. Part Second. London, 1851. 8vo, pp. 265-466.

On the Transmission from Parent to Offspring of some Forms of Disease, and of Morbid Taints and Tendencies. By James Whitehead, M.D., F.R.C.S, on the Medical Staff of the Lying-In Hospital, and Lecturer on Obstetric Medicine at Manchester, &c. &c. &c. London, Manchester: 1851. 8vo, pp. 351.

Lectures on the Physical Diagnosis of Diseases of the Lungs and Heart. By Herbert Davies, M.D., Fellow of the Royal College of Physicians; Senior Physician to the Royal Infirmary for Diseases of the Chest; Assistant-Physician to, and Lecturer upon Materia Medica at, the London Hospital; and formerly Fellow of Queen's College, Cambridge. London, 1851. Post 8vo, pp. 288.

On the Nature and Treatment of the Diseases of the Heart; containing, also, an Account of the Musculo-Cardiac, the Pulmo-Cardiac, and the Veno-Pulmonary Functions. By James Wardrop, M.D., &c., &c. London, 1851. 8vo, pp. 587.

Neuralgia: its various Forms, Pathology, and Treatment. Being the Jacksonian Prize Essay of the Royal College of Surgeons for 1850; with some Additions. By C. Toogood Downing, M.D., M.R.C.I. London, 1851. 8vo, pp. 375.

Translation of the Pharmacopœia of the Royal College of Physicians of London, 1851; with Notes and Illustrations. By Richard Phillips, F.R.S.L. & E., F.G.S.; late President of the Chemical Society; Curator of the Museum of Practical Geology, &c. London, 1851. 8vo, pp. 567. [Received on Monday the 8th December 1851.]

Journal fur Kinder Krankheiten. Herausgegeben von Dr. Fr. J. Behrend Prakt. Arzte und Mittglied Mehrerer gel. Gesellschaften, und Dr A. Hildebrand Konigs. Sanitatisrathe, und Prakt. Arzte in Berlin. Erlangen, 1851. Band XVI. Heft 5 und 6. Mai und Juni, 1851 8vo Band XVII. Heft 1 u. 2. Juli und August 1851.

Remarks on the Plea of Insanity, and on the Management of Criminal Lunatics. By William Wood, M.D., Licentiate of the Royal College of Physicians, and Medical Officer of Bethlehem Hospital. London, 1851. 8vo, pp. 70.

On Improving the Condition of the Insane. By Henry Monro, M.B. Oxon., Fellow of the Royal College of Physicians; Author of Remarks on Insanity; its Nature and Treatment, &c. London, 1851. 8 vo, pp. 23

A Brief Essay on the Nature and Treatment of Herniae or Ruptures, illustrated with Engravings. By David Tod, M.R.C.S., England &c. Sixth Edition. London, 1851. 8vo. Pp. 8.

The Principles and Practice of Obstetric Medicine and Surgery in reference to the Process of Parturition. With 120 Illustrations on Steel and Wood. By Francis H. Ramsbotham, M.D., F.R.C.P.L., Lecturer on Obstetric and Forensic Medicine at the London Hospital, &c., &c. Third Edition, enlarged. London, 1851. 8vo. Pp. 726.

Lectures on the Principles and Practice of Surgery. By Bransby B. Cooper, F.R.S., Senior Surgeon to Guy's Hospital, &c. London, 1851. 8vo. Pp. 964.

The London Medical Gazette, or Journal of Practical Medicine, No. 1242. New Series, No. 334. September 19, 1851, to No. 1255. New Series, 347. December 19th 1851.

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⁂ Communications and Books to be sent free to Messrs. LONGMAN & Co., London; or to Mr INCHES, Printer, Old Assembly Close, Edinburgh.

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—— 224, *for* " Denman, " *read* " Denham. "
—— 380, 17th line from top, *for* " opral, " *read* " spinal. "

NOTICES TO CORRESPONDENTS.

The following works have been received:—

Medical Lexicon: A Dictionary of Medical Science, containing a concise explanation of the various subjects and terms of Physiology, Pathology, Hygiene, Therapeutics, Pharmacology, Obstetrics, Medical Jurisprudence, &c.; with the French and other Synonyms; notices of Climate and of celebrated Mineral Waters, Formulæ for various Official, Empirical, and Dietetic Preparations, &c. By Robley Dunglison, M.D., Professor of the Institutes of Medicine, etc., in Jefferson Medical College, Philadelphia. Eight edition, revised and greatly enlarged. Philadelphia, 1851. Large 8vo. Pp. 927.

The Board of Health and the Cholera. A Memoir on the Saline Treatment. Worcester, 1852. Foolscap 8vo. Pp. 227.

Report of the Proceedings of the Pathological Society of London. Fifth Session, 1851-1852. London. 8vo. Pp. 196.

Medicina Mechanica, or the Theory and Practice of Active and Passive Exercises and Manipulations; considered as a branch of Therapeutics, and as adapted both to the Treatment and cure of many forms of Chronic Disease. By John W. F. Blundell, M.D. London, 1852. Post 8vo. Pp. 292.

Report of the Manchester Royal Lunatic Hospital, situate at Cheadle, Cheshire, in connection with the Manchester Royal Infirmary, from June 25th 1850, to June 24th 1851. Manchester, 1852. 8vo. Pp. 19.

Notes on Bright's Disease of the Kidney, as observed chiefly in the Clinical Ward of the Jamsetjee Jejeeboy Hospital at Bombay. By C. Morehead, M.D., Professor of Medicine, Grant Medical College. (Bombay Transactions, No. x.) 8vo. Pp. 46.

Homœopathy in 1851. Edited by J. Rutherfurd Russell, M.D. Edinburgh, 1852. 12mo. Pp. 416.

The Fallacies of Homœopathy, and the Imperfect Statistical Inquiries in which the results of that Practice are estimated. By C. H. F. Routh, M.D., M.R.C.S., Physician to the Saint Pancras Royal General Dispensary, &c. &c. London, 1852. 8vo. Pp. 85.

Researches and Observations on Scrofulous Disease of the External Lymphatic Glands. With Cases showing its connection with Pulmonary Consumption and other Diseases. By Thomas Balman, M.D., M.R.C.S., and L.S.A., &c., one of the Medical Officers of St Ann's Dispensary, Liverpool. London, 1852. 8vo. Pp. 189.

Physiology applied to Health and the Improvement of Physical and Mental Education. By Andrew Combe, M.D., Fellow of the R. College of Physicians of Edinburgh; Physician Extraordinary in Scotland to the Queen. Fourteenth Edition, Revised and Enlarged. Edited by James Coxe, M.D, Fellow of the Royal College of Physicians of Edinburgh. Edinburgh, 1852. Post 8vo. Pp. 345.

Sketches of Brazil; including New Views of Tropical and European Fever, with Remarks on a Premature Decay of the System incident to Europeans on their Return from Hot Climates. By Robert Dundas, M.D., Physician to the Northern Hospital, Liverpool; formerly Acting Surgeon to H.M. 60th Regiment; and for twenty-three years Medical Superintendent of the British Hospitals, Bahia. London, 1852. Post 8vo. Pp. 449.

Lateral Curvature of the Spine, its Causes, Nature, and Treatment. By R. W. Tamplin, F.R.C.S.E.; Surgeon to and Lecturer on Deformities at the Royal Orthopædic Hospital, Bloomsbury Square, &c. London, 1852. Large 8vo. Pp. 44. Four Lithograph Figures.

The Stomach and its Difficulties. By Sir James Eyre, M.D., Edinburgh; Licentiate of the Royal College of Physicians of London; Consulting Physician to the St George's and St. James's Dispensary, &c. London, 1852. Post, 8vo. Pp. 152.

A Letter to the Right Hon. Sir George Grey, Bart., M.P., her Majesty's Principal Secretary of State for the Home Department, &c. &c., on Medical Registration, and the present Condition of the Medical Corporations. Second Edition, with Notes and Abstract of a Proposed General Medical Registration Act. By Emeritus. London, 1852. 8vo. Pp. 38.

Varicose Veins and Varicose Ulcers. By Thomas William Nixon, Surgeon to the Western Dispensary, Demonstrator of Anatomy at the Middlesex Hospital. London, 1852. Post 8vo. Pp. 63.

Fourth Report of the Somerset County Asylum for Insane Paupers. From 1st January to the end of the year 1851. Wells, 1852. 8vo. Pp. 60.

Report on the Mortality of Cholera in England, 1848--49. London, 1852. Large 8vo. Pp. clxxiv. and 300. From the Registrar General.

Asylums for the Insane. Observations upon the importance of establishing Public Hospitals for the Insane of the Middle and Higher Classes; with a Brief Exposition of the Nature of Insanity, and the Present Provision for the Treatment of the Insane. By Thomas Dickson, L. R. C. S. E. &c. London, 1852. 3vo. Pp. 62.

On True and False Spermatorrhœa. By Dr Pickford, of the University of Heidelberg. Edited by Chirurgus. London, 1852. 8vo. Pp. 82.

Researches upon the Pathology and Treatment of Deformities of the Human Body. By John Bishop, F. R S., Member of the Council of the Royal College of Surgeons of England, Consulting Surgeon to the Northern Dispensary, late Senior Surgeon to the Islington Dispensary, &c. &c. London, 1852. 8vo. Pp. 266.

The Hand; its Mechanism and vital Endowments, as evincing Design. By Sir Charles Bell, K.G.H., F.R.S., L. et E. Fifth Edition Revised, with Woodcuts. London, 1852. Post 8vo. Pp, 428.

Observations in Surgery. By Benjamin Travers, junior, F.R.C.S., lately Resident Assistant Surgeon at St Thomas's Hospital, and Lecturer on Surgery, &c. &c. London, 1852. 8vo. Pp. 230.

The Principles and Practice of Surgery. Illustrated by Numerous Engravings on Wood. By William Pirrie, F.R.S.E., Regius Professor of Surgery in the Marischal College and University of Aberdeen; Surgeon to the Royal Infirmary, &c. &c. London, 1852. 8vo. Pp. 952.

Thomson's Conspectus of the British Pharmacopœias. Sixteenth Edition. Edited by Edmund Lloyd Birkett, M.D., Cantab. Fellow of the Royal College of Physicians, Physician to the City of London Hospital for Diseases of the Chest, Curator of Museum, Guy's Hospital. London, 1852. 12mo. Pp. 214.

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The London Journal of Medicine. No. 37, January; 38, February; 39, March. 1852.

The American Journal of Medical Sciences, New Series. No. 44, October 1851, and No. 45, January 1852. Philadelphia.

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Weekly Tables of Mortality for the Metropolis, No. 51, week ending Saturday, December 20, 1851. No. 52, week ending December 27 to No. 12, week ending Saturday, 20th March 1852.

Journal für Kinderkrankheiten. (Journal for Diseases of Children). Herausgegeben von Dr Fr. J. Behrend und Dr A. Hildebrand, Erlangen 1851. Band xvii. Heft 5 und 6. November und Dezember 1851.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1st JANUARY 1852.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Hæmorrhagic Diathesis exemplified in two Brothers; with Remarks on its Hereditary Tendency.* By Dr Pickells of Cork.

These cases occurred in the country, at a place called Cloghroe, about seven miles from this city. The elder brother, aged about 20, had been reaping during a hot day, in harvest, when on coming to the end of a furrow or ridge, in endeavouring through a spirit of rivalry to out-do his fellow-reapers by being the first to put his sickle in a new ridge—a sort of rivalry often encouraged in their avariciousness by employers or hard task masters; his reaping hook striking against a stone, glanced aside, and wounded him in the wrist. He died of the hæmorrhage on the same day. The younger brother, a lad of about 14, happened in about a year after to cut himself slightly with a penknife in the thumb. The wound was bound up in the usual way, and the hæmorrhage appeared to have ceased, when on the third day after the accident, making some slight use of the thumb, it broke out afresh, and he died in the course of the same day.

Regular medical assistance was not obtained in either case; but various simple styptic applications, such as are used in domestic medicine, were resorted to. The elder brother had been placed in a sort of easy rustic vehicle, called a butt, for conveyance to one of the surgical hospitals of this city, but sank before he reached his journey's end. They were both of a thin habit, with a flush or degree of floridity on the cheek, and not unhealthy. They had previously shown no disposition of the kind, nor has the tendency appeared in any of their relatives. A married brother, however, became so alarmed, that for a long time after he avoided handling, or at least using, a sharp implement of any sort, even obliging his wife to cut his victuals for him at meals. The neighbouring peasantry, perplexed at the same time, and shocked by their sudden and untimely fate, physiologizing in their own ignorant way, endeavoured to account for it by supposing "they had no veins." This report has been derived from information got from a first cousin of the deceased parties, who had opportunities of learning all particulars.

The knowledge of the fact of the occasional existence of this diathesis is of practical utility, as showing the necessity of caution on the part of the medical practitioner in the treatment of the diseases of families, who may, on inquiry, be found liable to it. Owing to this melancholy peculiarity of constitution, instances have occurred of fatal or nearly fatal hæmorrhages under the most skilful hands, from even the ordinary minor surgical operations; as, from cupping, Dublin Medical Journal, vol. vii.; leeching, ibid; from scarifying, Edinburgh Medical and Surgical Journal, vol. xii.; the seaton, Lancet, vol. i.; the extraction of a tooth, ibid., and Edinburgh Medical and Surgical Journal, vol. xii.; the constitutional predisposition in some individuals being so great, as that the slightest cut or scratch has been sufficient to develope and call it into action, "biting the tongue;" "a slight cut with a penknife," as in one of the Cloghroe cases; "a cut in shaving;" even "the prick of a pin."*

The fatally profuse epistaxis, defying every means of checking it, which has in some very rare instances taken place in common fever, may not unfairly be ascribed to this cause; the symptom being of itself rather salutary than otherwise.

Mr Wardrop, in his lectures on the sanguineous system, Lancet, vol. i., in remarking on the few cases of this diathesis recorded in medical works, has, both from his own observation and inquiries, and from medical works, collected a number of instances. In no instance, it is remarkable, was the operation of vaccination, whether by punctures or incisions, attended with untoward effects.

The first notice we have of the disease is in a work in Arabic, published by an Arabian surgeon, resident in Spain (Albucasis) during the dominion of the Moors in that country.

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* After the above had been sent to press, the following case taken from the Bath Chronicle, October 26th, occurred in Bath. "A young woman, who resided in Philip Street, died in a somewhat curious manner. In the course of the day she observed blood oozing out of one of her legs; but as the aperture was not larger than the puncture of a pin, very little notice was taken of the circumstance. Afterwards, however, becoming faint, her friends removed her to the Bath United Hospital, when it was discovered that she had ruptured a blood vessel, by *tying*, as was supposed, *her garter too tight*; and, notwithstanding the skill applied, she died from exhaustion soon after removal to hospital."

The most remarkable characteristic of the disease is its hereditary tendency. In this respect some of the instances on record possess a fearfully tragic and momentous interest. Thus in a case given, (Edin. Med. and Surg. Journal, vol. xxv.), which occurred near Bristol, it was stated by the mother of the child who died, that none of the family had, during some generations, reached to man's state, and that they had all of them been subject to profuse hæmorrhages from the most trifling wounds. Her own brother had died of hæmorrhage succeeding the extraction of a tooth. The females enjoyed good health, and were entirely free from the tendency to bleed excessively from superficial wounds, but if they married their sons inherited from them this family curse, whilst their daughters were equally free with themselves from it. The law in this respect appears to be very singular, a sort of penal (if the term be allowed) salic law, entailing the disease, in its hereditary character, on the male branches of a family to the exclusion of the female; yet transmitting it through the females to the males of the next generation.

In Mr Wardrop's paper, the following case is given on the authority of a medical practitioner in York, as illustrative of the hereditary tendency of this diathesis, in which it was hereditary in the male branches of the same family, and confined to males. "A boy labouring under this diathesis had five uncles and two aunts; all his uncles had the same hæmorrhagic tendency; the aunts had not that tendency in their persons. The one had three boys, two of whom are thus affected; the other has two boys and two girls, both brothers are affected in the same way. The mother of the boy whose case is given "had a numerous offspring, and a brother, who is 22 years, is affected in a similar way, and is an almost constant sufferer from rheumatic gout."

An instance still more striking is that published by Dr Nasse, (occurred in Germany) in Hufeland's Journal, as extracted in Edinburgh Medical and Surgical Journal, vol. xxv. " A woman, whose paternal uncle had been subject to epistaxis, had lost all her five brothers in early youth by hæmorrhages from trifling injuries, or without ascertained causes. She herself had five sons and two daughters. Of the sons, four had died of hæmoptysis. None of the females of the family had shown any hæmorrhagic disposition.

In article " Hereditary Diseases," in Cyclopædia of Practical Medicine, a minute history, taken from a German journal of cases which occurred in Oldenberg, is given by Dr Riecken. The parents had never been subject to hæmorrhage; and the father, Ernest P., was living in good health, in his 88th year, at the time of the publication of the narrative. The couple had twelve children, five sons and seven daughters, of whom three boys and one girl died of hæmorrhage. Their youngest daughter, who never suffered from the disease, married a healthy man, and had six children, four boys and two girls, of whom three boys died of hæmorrhage. Ecchymoses were to a remarkable extent a concomitant of these cases. None of the ancestors or collaterals of Ernest P. had been affected with the disease.

In vol. xxxiii. of London Medical and Physical Journal, the history, taken from the New England Journal, is traced for a century, of a family who, from the many individuals of it who were cut off by fatal hæmorrhages from slight causes, went by the name of the "*Bleeders.*" The son of the reporter, Dr Hay, had intermarried into the family. Mr Oliver Appleton, the first of the family mentioned, had been subject from his youth to profuse bleedings from slight causes. In advanced life, an hæmorrhage taking place from the urethra contributed to his death. He had two sons, both physicians, who both bled to death. He left three daughters, two of whom married into a family by the name of Swain. Doctor Thomas Swain married the elder, and had by her two sons, Dr Oliver Swain, and Dr Thomas Swain, who both bled to death. General Brown married the eldest daughter of Dr Thomas Swain, Junior, and had by her three sons, one of whom bled to death at the age of fifteen. The diathesis is then traced downwards in the descending line and its collaterals, the reporter laying down, from an induction of facts, the aphorism, that "The children of bleeders (the daughters) are never subject to this diathesis, though their grandsons by their daughters are." Nati natarum, et qui nascentur ab illis. Dr Hay states that he had attended three or four persons of this idiosyncrasy, and constipation of the bowels being a never-failing evil among them, he had found no article answer better than an ounce of sulphate of soda, administered for two or three days in succession, it generally stopping the hæmorrhage; a more frequent repetition of it, however, being certain of producing the effect. "Persons subject to this indisposition are," according to him, "of a florid complexion, remarkably healthy, and "extremely irascible." In other instances, however, they are described as being of dark complexion. The Cloghroe brothers were, as has been stated, "of florid complexion, and not unhealthy." The minute history given by Dr Hay, painfully interesting in its details, is further interesting, as tending to show the value of a suggestion of Dr Laycock in his lectures on the hereditary transmission of diseases (Lancet, 25th November 1843), namely, "That every family should keep a register or domestic history of the family diseases." As most hereditary diseases are so by predisposition, by avoiding, so far as avoidable, the known exciting causes, as where hæmorrhagic diathesis exists,—the unrestrained indulgence of the passion of anger, the predisposition may perhaps not be called into action, and the development or manifestation of the disease may be thus prevented.*

When fatal, it has often terminated with frightful rapidity,—life ebbing within a few days from commencement of attack; in the instance of the Cloghroe brothers, within a day or two. In other instances the disease has been protracted, the hæmorrhage continuing to recur at intervals more or less distant for months or even years. Of this protracted form are examples, a case in Part Second of Boates's Natural History of Ireland, entitled "Periodical Evacuation of Blood at the End of the Forefinger," and another in Vol. v. of Philosophical Transactions, abridged, entitled "An Extraordinary Periodical Hæmorrhage from the Thumb." These cases have not, I believe, been noticed in medical works. In the first-mentioned, the patient was an inn-keeper in Trim, county of Meath, of sanguine complexion. With him the hæmorrhage did not begin till the forty-third year of his age (coming on about Easter), but continued to recur at frequent intervals, seldom giving a respite of two months during the remainder of his life, which was prolonged to twelve years. The man met with no outward accident which might at first have brought it on him, it having been a spontaneous act of the constitution. He rarely bled less than one pottle at a time. The blood would spin out from the end of the forefinger, the right, in a violent but small stream. After it had spent its violence, it would cease for a time and then only drop, then spring out again with violence, continuing thus for twenty-four hours, till at last he fainted away, when the blood stanched of itself, and his pains left him. Drinking more than ordinarily made him more apt to bleed. He died of the disease.

This case confirms a statement of Dr Hay, that age does not exempt. The other case was that of a person who, from his earliest years, even, as he heard, from his infancy, had been subject to the hæmorrhage (from the right thumb), the time of the eruption being about the full of the moon. Under this discharge, however copious, (when he came to be sixteen, the quantity had increased to half a pound at each eruption), he was strong and vigorous to the age of twenty-four. At that age, finding the evacuation troublesome, and being uneasy under it, he seared with a hot iron the orifice which used to give it vent. The searing had stopped the hæmorrhage for about twenty years. The consequences were, however, very serious, for within one quarter of a year after the operation, he was seized with severe hæmoptysis, spitting up vast quantities of blood by coughing. This was arrested by frequent blood-letting, &c., yet not with that relief which was expected; for, in a very short time after, a most violent colic supervened,—a disposition to which, though in a good measure overcome by purgatives, still remained at the date of the account, being often troubled with it, as also with a spitting of blood on the least excess of cold or motion.

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* Among diseases which run in families, may be included, there is reason to apprehend, that direst of maladies, the Asiatic cholera is to be placed. A clergyman, now residing in the neighbourhood of this city, but then residing in a distant part of the county, informed me, that, in 1832, he knew an instance of four members of a family living in different places, having been all attacked with cholera without the slightest communication with each other. In Persia, I have somewhere seen it stated, certain families of the nobility doomed, as it were, by a peculiar constitutional liability, have been rendered extinct, or almost extinct, by the malady, which, from its repeated visitations, threatens to become domiciliated among us.

From this and other instances of such like hæmorrhages in men, the medical reporter, Dr Musgrave, was led to infer that these evacuations, being analogous to the *menses* in women, confute the opinion of all such as derive that discharge from a *fermentum uterinum.* "For," he asks, "how can we think the *menses* come from such a local ferment, when a discharge in all respects equivalent to them takes place where no such ferment is, or can be supposed?" Of late years, the doctrine has been broadly maintained by Gall of "a periodic movement in the male system, analogous to that which returns monthly in the female;" "the hæmorrhage," according to Chomel, "recurring sometimes at precisely regular intervals, and by monthly periods more commonly than any other; its accidental interruption being almost uniformly the cause or the effect of some derangement of the health."*

In the protracted cases now instanced, the hæmorrhage was confined to one part (the finger). In the volume already referred to of the Philosophical Transactions is a very remarkable case by Dr Mesaporiti of Genoa, entitled, "An Eruption of Blood from Several Parts of the Body." Among other parts, the blood oozing or escaping, as has happened in some, though rare, cases of the hæmorrhagic diathesis, even by the pores of the skin (hæmorrhage by exhalation). The original is in Latin (ex utrâque velaminum, et ex oppositá parte.)

A young lady, aged eighteen, of singular beauty, after a stomach complaint of long standing, began early in April to spit up blood by coughing, labouring under the pleuritic symptoms of pain in side, and difficult respiration. On the fourth day of the attack, the pectoral affection ceasing, epistaxis set in, for which the surgeon took blood from the foot, as he had on the first days of the attack twice taken it from the arm. The disease not abating, vomiting, first of bile, and afterwards of blood, came on; the constant vomiting and loss of blood being most distressing. To meet this symptom, besides internal means resorted to, including astringents and anodynes, blood was taken from the *vena salvatella*. The disease, notwithstanding, still proceeding, the symptoms now mentioned were exchanged about the beginning of June for a new and complicated train,—bleeding from the ears, from the tops of the fingers and toes, from the navel, from the angle of the eye; several times by sweat; then from the middle of the chest; afterwards from the foot, there where the saphene vein is pressed on (tunditur); lastly, from both wrists, and the parts opposite; from the chin, and, during nights, from the tip of the tongue. These anomalous and erratic symptoms all completed their course within the space of a fortnight. About the middle of June, expecting benefit from change of air, she removed to the country, but returned early in August without amendment, or even worse; the hæmorrhages still taking place, sometimes from one part, sometimes from another, but most frequently by sweat, during the remainder of that month, and up to the middle of September, towards the end of which month the symptoms had all much abated or ceased. Thus the disease, with occasional intermissions, in one instance of a week, in another of twenty days, at the time of menstruation (the menses not having been suppressed), had continued nearly six months; the hæmorrhages during that period having been from so many and so various parts, that she may be almost said, without hyperbole, to have "bled from every pore." The symptom latterly found most distressing was the partial sweating of blood; her chemise, or inner garment (indusium), used to be stained from the dew of blood which exuded on the skin, particularly over the chest. What renders this case the more remarkable in a physiological point of view is, that the menses had been always regular and in sufficient quantity; the rare forms of hæmorrhage, and those of the skin in particular, being, in almost every instance, vicarious of, or supplemental to, suppressed menstruation,—a revulsion taking place, and the congestion finding vent by some devious outlet, as in an instance mentioned in one of the volumes of the London Medical Gazette, in which a young girl, who had never menstruated during a paroxysm of ague, sweated blood by the feet.

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* There is an apparent forcing of facts, procrustes-like, by Chomel, to suit his hypothesis. The straining of isolated facts, in pursuit of an hypothesis of this sort, might seem to some to merit the sarcasm applied by Reid in his essays to certain theorists, as, in their neomania, or love of paradox, "Hunting after bladders of soap in the land of chimera."

On looking over reports of cases of the hæmorrhagic diathesis, the reader is struck by its frequent combination with ecchymoses, a combination such as might at first view lead to the supposition of its being identical with *purpura hæmorrhagica.* In vol. xxvi. of the Edinburgh Medical and Surgical Journal, is an article by Alexander Murray, surgeon at Alford, entitled "Cases of *Hæ-*

morrhea petechialis in individuals related to one another." *Hæmorrhea petechialis*, the term originally applied by Dr Adair, and by Dr Duncan, Senior, to *purpura hæmorrhagica*, appears to me to have been in this instance a misnomer; the cases appearing to be illustrative not of *purpura*, but of the hæmorrhagic diathesis. Mr Murray, too, identifies them as cases of the same disease with those which had been described in the previous volume of the Edinburgh Medical and Surgical Journal. The individuals were five boys, of whom three died.

The hereditary tendency, or family predisposition to fatal hæmorrhages has not, I believe, been remarked as accompanying purpura. I have treated from time to time about fifty cases of *purpura simplex* and *p. hæmorrhagica;* but do not recollect that in any instance mention was made of such tendency or predisposition in individuals of the family. Purpura, far from being as is (it would seem in its hereditary transmission), the hæmorrhagic diathesis, strictly entailed on males, in a majority of cases, as stated by writers, affects females, a statement, the correctness of which I can confirm from my own observation. Some affinity between the two diseases there may be, but they cannot, I think, be considered identical. The ecchymoses of purpura are sometimes combined or complicated with small-pox, these symptoms being in such cases regarded as accessary or adventitious. Of this combination I have witnessed instances. Confluent small-pox, when combined, as I have seen it, with *purpura hæmorrhagica*, presents a spectacle as hideous as hopeless; hæmorrhages emanating from the different outlets, as in the " bloody small-pox" of Mead and Sydenham. The ecchymoses of purpura may be distinguished from the petechiæ of typhus by their in general larger size, as well as mode of distribution, dispersed at intervals over the surface, studding, as it were, without being elevated, the skin. Another striking fact, observable in reading the reports of cases of hæmorrhagic diathesis, is its frequent combination with rheumatic gout. Hence Dr Riecken, in his general remarks on the history of the cases in the family of Ernest P., was led to attribute the constitutional hæmorrhagic diathesis to a gouty taint. This idea is probably not well founded; as, if the diathesis were dependent on gout, cases would be oftener met with.

The pathology of the diathesis is involved in much obscurity. The proximate cause usually assigned is a want of power of coagulation in the blood; the blood, when drawn, concrete or gelatinous, and red at first, becoming afterwards thin and pale, or watery. This thinness of the crasis of the blood is, however, met with in some other diseases, as also in certain cases of sudden death from external violence; for similar morbid conditions of the blood may arise from different causes.

Dr Mead, in his Essay on the Small-Pox, arguing from the analogy of the symptoms in "the bloody small-pox" to those produced by the bite of the Lybian serpent, *hæmorrhois*, so graphically described in the passage of Lucan, infers it as evident that "these horrid symptoms in the small-pox are the effects of an acrid poison,"—a species of reasoning which might apply with equal force to the etiology of the hæmorrhagic diathesis, the appalling phenomenon sometimes taking place, as in the bloody small-pox, of hæmorrhages from all the organs. The beautiful passage of Lucan, turned into English, is thus:—

> "A fierce hæmorrhois struck both his fangs
> Deep into Tullus,—a brave, valiant youth,
> And fond admirer of great Cato's worth.
> And as Corycian saffron, when 'tis squeezed,
> Pours forth its yellow juice through all the holes
> Of the hard pressing boards; so from the pores
> Of all the parts flowed ruddy-venomed gore.
> His tears were bloody; nature's passages,
> For their own humours were all filled wlth blood.
> His mouth, his nose, choked up with filthy clots:
> Red sweats transpired from all the skin inflamed.
> His body seemed one universal wound."

Similar effects are ascribed to the bite of the rattle-snake, sometimes copious bloody sweats, often hæmorrhages from the eyes, nose, and ears.

The poison of the *serpens hæmorrhois* was, however, it would appear, not invariably fatal nor incurable, Celsus having prescribed a formula for the cure of it. Climate would, however, make much difference in this respect; the poison of serpents being most noxious in very hot and arid climates. Sallust, in his history of the Jugurthine war, speaking of an African town, to the siege of which the Romans were about to march, says, "It was more impregnable by its situation than by its walls, and arms, and men, with the exception of the immediate vicinity of the town, all else a vast desert, without water, and infested by serpents; the violence of which, as of all wild animals, is rendered fiercer by want of food. Add to this the nature of serpents, deleterious of itself, is more inflamed by thirst than by any other thing." The symptoms of hæmorrhage from the mouth, and ecchymoses on the skin, are often, too, it is stated, the effects of the poison of hemlock. How much the action of poisons on the human economy is modified by a full or empty state of the stomach, is pointedly exemplified in the circumstances of the death of the twenty-eight senators of Capua, who, in the second Punic war, poisoned themselves, as recorded by Livy, rather than fall into the hands of the Romans, incensed by the revolt of that city to Annibal. "At a feast prepared at the house of the senator who had been the author of the revolt, after they had been satiated with meat and wine, a cup, containing a poisonous potion, was carried round. With minds alienated, so far as possible, by wine, from the sense of impending evil, all took the poison (hemlock, it is to be presumed)." * * * * * * "The veins, replete with the meats and wine, rendered the force of the poison less efficacious in hastening death. Most of them lingered throughout the night and part of the following day. All, however, before the gates were opened to the enemy, expired."

In mentioning hemlock, I take the opportunity of correcting an inaccuracy, into which I find I fell in an article published in a former volume of this work (vol. lxvii.), entitled "Deleterious Effects of *Œnanthe Crocata*, or Hemlock-Water Dropwort, as it occurs in the south of Ireland."

Speaking of the *Atropa Belladonna* (deadly nightshade) now fortunately become vary rare, I said it was to be found at the East Ferry in the Harbour of Cork. I should have said the *Solanum Nigrum* (black nightshade).

ART. II.—*History of a Case of Epilepsy, accompanied with Fever, Inflammation, Perforation, and Gangrene of the Lungs, and also Pneumothorax.* By ROBERT SCOTT ORR, M.D., formerly Resident Superintendent of the Glasgow Royal Infirmary.

JANET DUNCAN, aged 24, was admitted into the Royal Infirmary, in the month of December 1839, at which time I acted as clinical clerk to Dr Alison.

From what could be ascertained from her friends at that period, and subsequently, the following appears to have been the correct history of her case.

About ten years before her admission into the hospital, she was attacked in the middle of the night with a fit, which, from the account given of it, and the frequent recurrence of similar fits afterwards, seemed to have been of the nature of epilepsy. She was for about a year after this afflicted with a fit generally once a month; there was at that time never more than one in number, and it lasted generally only about one minute; previously and subsequently to its occurrence she suffered a good deal from headach. During the fit her limbs were convulsed; her eyes turned upwards; her tongue was clenched between her teeth, and was frequently severely bitten; she moaned and sometimes cried, after which she always fell asleep; and next morning (for these attacks always occurred at night) she was unconscious of having been ill.

After she had been thus subject for about twelve months to these attacks, an interval of seven years elapsed, during which she remained quite free from them; the year following that, however, she was again in a similar manner affected.

For twelve months before she came into the hospital the catamenia had been very scanty and irregular; and she complained much of headach, with latterly sense of tightness across the forehead, and appearances as if balls of fire were passing before the eyes. The fits at this time also had increased in the frequency of their attacks, occurring once a fortnight; but subsequently to the application of leeches to the head, they supervened only once in six weeks.

The week before her admission she had three fits each night for three successive nights; and on the night of the 11th of December she slept little, and complained much of pain in the head. On the morning of the 12th she ran away from home, and came into the Infirmary in quite a maniacal state, complaining of uneasy feelings, and crying to have the head shaved and blistered.

She was accordingly put under the care of Dr Alison, who directed that she should be bled from the arm to the extent of twelve ounces; that she should take half a grain of tartrate of antimony, dissolved in one ounce of water, every hour, and that cold applications should be applied to the head.

The following reports were taken by Dr Alison of her state aud symptoms while she remained in the house.

"*December* 13*th*. Twelve ounces of blood quite natural. Had several doses of antimonial solution, and soon became quiet and taciturn. One scanty stool from powder of jalap and calomel. The pulse was of natural frequency, rather small; the face is pale; skin cool; no vomiting.

"Cold applications to the head to be continued; and if stupor persists, a blister to be applied over the vertex in the evening.

"14*th*. Blister was applied last night, and had two pills and a senna draught this morning. Blister has risen and medicine operated. Speaks sensibly this morning. Pulse 78, soft. No spasmodic symptom.

"Continue antimonial solution, if delirium returns.

"15*th*. Bowels well moved again last night; had a quiet night. Pulse rather slow; takes a little food; tongue dry.

"One ounce of Epsom salts to be dissolved in eight ounces of water, with one drachm of dilute sulphuric acid, and one ounce of the solution to be given every third hour until the bowels are moved.

"16*th*. Had a quiet night; medicine has not operated. The solution of sulphate of magnesia to be continued.

"17*th*. Medicine operated; head is easy; slept quietly.

The solution of sulphate of magnesia to be repeated to-morrow morning.

" 18*th*. Had a quiet night. No headach to-day. Is still taciturn. The acid solution of Epsom salts to be continued; and the feet to be placed in warm water in the evening.

" 19*th*. Had an easy night; no headach; pulse natural; still very taciturn.

Over the head to be applied a lotion of tepid water and vinegar. To take two of the pills of aloes and iron twice daily; and the feet to be placed in warm water in the evening.

" 20*th*. Had a quiet night. Bowels open. The pills of aloes and iron to be continued.

" 22*d*. Pediluvium to be repeated, and pills to be continued.

" 23*d*. Had some headach last night, and slept little; pulse natural, 78; bowels open.

" Eight leeches to be applied to the temples.' To have, at the hour of rest, a draught containing one drachm and a-half of tincture of hyoscyamus.

" *January* 4*th*, 1840. Continues easy and sleeps well. Bowels kept open without pain."

She was now dismissed with directions; and was recommended to take two of the pills of aloes and iron twice daily. Of these she received at dismissal three dozen. She was dismissed much relieved, not having had any return of the fits since the time of her admission.

On the 8th of January I was requested to visit her at her own home, and found her much in the same state, in which she was when she left the hospital.

On the 15th I again saw her. The catamenia had not then returned, and indeed they never afterwards did return. She had had a fit each of the previous nights, lasting about one minute; and had since that occurrence complained of headach; although I had never seen her during a fit, still I could not doubt that they were truly epileptic. Notwithstanding the supervention of these attacks, she was ordered to continue the use of the pills of sulphate of iron and aloes, and had no more return of them till February 4th, when she had a slight fit.

For above seven months she continued pretty well, with, however, occasional fits at intervals. But on the 11th of September she had a number of severe attacks, amounting, it was stated, to ten or twelve. Dr Alison saw her and bled her; ordered tartar emetic ointment to be rubbed on the head; gave her tartar emetic internally, and also pills, containing extract of colocynth, hyoscyamus, and croton oil. I had been previously sent for, but not being at home, Dr Alison was gone before I arrived. I however saw her immediately after he left. The face was then pale; the eyes were heavy; the arms were pretty rigid; and she was quite taciturn and incoherent.

On the 13th she was still taciturn. Two pills produced only one motion. The pulse was 86; the arms were rigid; the tongue was covered with a whitish moist fur. Two more of the pills were given, and the tartar emetic frictions were recommended to be continued. Dr Alison advised the use of small doses of turpentine, and entrusted the patient to my care.

14th. Tongue still foul, and bowels still sluggish.

Two more pills were ordered, and the following mixture:—

Oil of turpentine half an ounce; mucilage of gum arabic four ounces; syrup of orange pill three drachms; water four ounces. To take one ounce of this mixture three times daily.

17th. Had a very restless night, with constant talking and jactitation. Bowels confined; tongue foul; and breath heavy. Is at present restless, with frequent puffing motion of the mouth; limbs rigid, especially left arm. She is quite insensible.

The hair to be removed, and a blister to be applied to the scalp. To take immediately two drops of croton oil, two grains of hyoscyamus, and one grain of compound extract of colocynth in the form of pill.

Vespere. Bowels still unmoved; still restless and talkative; arms rigid; pulse 120, small; tongue foul and dark-coloured.

An enema with one ounce of oil of turpentine to be administered. The antimonial solution to be continued.

18th. Blister rose well; is at present asleep, and has been quite quiet. One motion passed unconsciously. Pulse 88, small and feeble; rigidity of limbs gone; surface of the body warm and perspiring.

A domestic enema to be administered.

Vespere. Is now perfectly quiet. Pulse 76, of better strength. Tongue still covered with a fur, but it is whiter and much moister; it is considerably injured by the teeth. The patient complains of hunger, and is much inclined to sleep; head covered with a copious crop of pustules from the tartar emetic ointment. When asked if she felt better she replied, "Yes," and protruded the tongue when desired.

Enema not given.

19th. Enema not given hitherto. The pulse is more frequent, but she is more rational and can name those around her. One motion. Tongue covered with a white moist fur. She complains still of hunger, although she has had food, and is still much inclined to sleep. She says she is "sick, sick."

A domestic enema to be administered.

20th. She continues sensible, though still somewhat incoherent; tongue furred; two motions from enema; pulse 96.

To have a draught of infusion of senna.

21st. Still somewhat incoherent and restless; pulse 86, of good strength; appetite good; bowels well opened by senna infusion.

Three grains of tartrate of antimony were dissolved in three ounces of water; and of this solution she was directed to take half an ounce every second hour.

23d. Is now perfectly quiet and rational, and understands what is said to her. Pulse 90, of good strength; appetite good; tongue much cleaner; complains now only of some headach; bowels open, and she is now conscious when the stools are passed.

30th. Is now quite well and perfectly sensible, her intellect not being seemingly impaired. The appetite is good. She continues the use of the tartar emetic ointment, and attention to diet was strictly enforced.

Nearly five months elapsed after this date before I again heard anything of this young woman. On the 27th of February 1841 I was however sent for to see her, and on my arrival I was told that since the previous day she had had nine fits. She was when I saw her stupid, drowsy, and rather restless; but when roused she answered questions, and complained of headach. The tongue when protruded was furred; the pulse 70, full; the bowels said to have been freely opened by medicine.

I directed the following medicines to be given.

Three minims of croton oil and eight grains of aloes were directed to be made with aromatic confection into four pills; and of these one was to be given every fourth hour till the bowels were well moved.

A solution of tartrate of antimony was also directed to be prepared, consisting of three grains of the salt in six ounces of water, with two drachms of solution of muriate of morphia; and of this a table-spoonful was ordered to be given every two hours while the patient was restless.

That same evening she had three more fits; and when I saw her next day she was still drowsy, but sensible when roused; the pupils were slightly contracted; the pulse 88; the tongue furred and dry in centre, and the headach continued. She had taken only one pill, and that without effect. In the evening, being much in the same state, and another pill taken having produced no effect, the pulse being from 94 to 96, sharp and full, and the drowsiness continuing, I bled her from the arm to the extent of 18 ounces, and afterwards gave her another croton-oil pill, with directions that, if this failed to move the bowels, an enema, with an ounce and a half of turpentine, was to be administered. The head was also to be shaved.

Next day, March 1st. The blood drawn was slightly cupped, but not buffed. She was, however, still stupid and listless, protruding the tongue when desired, but forgetting to draw it in again. The pulse was 100, much smaller. One stool had been passed consciously. The tongue was nearly clean. The urine was voided unconsciously. She had been rather restless in the night, but had no return of the fits.

A cathartic draught was ordered, and the antimonial solution to be continued.

On the 2d she was still taciturn. The tongue was dry and furred; the pulse 88. There had been no more fits, and there was no rigidity of the limbs. One motion had taken place in the night. A teaspoonful of morphia had been unadvisedly given; she slept well.

A croton oil pill was ordered, and afterwards a cathartic draught. The antimony to be continued.

3d. Taciturnity continues; pill produced one motion; pupils natural; pulse 88; tongue dry in centre, but she forgets to draw it in again; some thirst; she is rather more intelligent.

To two drachms and a half of the compound jalap powder were directed to be added half a drachm of carbonate of soda, and fifteen grains of powder of scammony; the whole to be divided into three powders, one of which was to be immediately given; and if the bowels were not freely moved, to be repeated in the morning.

4th. Much in the same state; pulse 88; one motion passed unconsciously. Two powders taken.

The third powder was ordered to be given, and a blister to be applied over the scalp.

5th. Blister rose well; less taciturn, and answers questions; two involuntary motions; tongue cleaner and moister; pulse 70; slept well; no fits.

To have to-morrow morning one ounce of castor oil.

9th. Quite rational; bowels open; pulse natural.

On the 27th of April I was called to see her. She had had fits at intervals for the two previous weeks. The night before I saw her she had had five. She was restless and drowsy, but answered questions slowly, and said she had no headach. The pulse was about 80, and the tongue foul.

She was directed to continue the use of the croton oil pills and antimonial mixture formerly prescribed.

On the 9th, 10th, and 11th of May she had a fit each day, but was otherwise well, except that some degree of acidity at the stomach was complained of.

Having at this time seen various cases of epilepsy treated by Dr Craigie with the powder of indigo, with the effect of reducing the number and frequency of attack of the fits, I was induced by his advice to try the effect of this remedy on the individual whose case I am now relating, and accordingly administered it in the following manner.

Indigo powder, ten grains; aromatic powder, five grains; carbonate of soda, three grains. The whole to be carefully mixed. This powder to be taken twice daily.

24th. No return of fits; urine and stools said to be of a dark blue colour.

30th. She had one fit.

Continue the indigo powder.

On the 11th of June she had several severe fits, but was otherwise so well as to be able to be out walking in the evening. On the 12th she had several still more severe.

I was requested to see her on the 13th, and found her quite insensible, but restless; the respiration was hurried; the pulse 94, full; the pupils very much contracted. The eyes were suffused, and the countenance exhibited a febrile aspect. The bowels had been freely moved by the administration of salts and senna, and she had had two table spoonfuls of the antimonial solution. She was immediately bled to the extent of 18 ounces, and an enema, with two ounces of turpentine, was ordered, which was to be repeated if necessary. At 4 P. M. she had another fit. I saw her immediately afterwards, and the pulse was then 120. The enema operated well, but during the night she had upwards of twenty fits.

On the 14th she was still insensible. The pupils were much contracted. The respiration was 48; the pulse 128, much smaller; tongue dry; countenance white and blanched; and she was very somnolent.

A large blister was applied to the head, and one ounce of turpentine, with half an ounce of mucilage, was given by the mouth.

15th. Pulse 128; blister rose well; no return of fits; insensibility continues; pupils rather more dilated; respiration natural and easy; countenance also more natural; still somnolent; when asked to put out the tongue she resists, and firmly closes the jaws. She had about 2 P. M. some loud hysterical crying and restlessness. Bowels stated to be open, a copious motion of a greenish colour having taken place at 2 P. M.

To take immediately six grains of calomel and one scruple of powder of jalap in treacle; and to-morrow morning four ounces of the cathartic draught.

16th. Pulse still 128; but she is less drowsy, and opens the eyes; does not however protrude the tongue or answer questions when desired; two motions. On the sacrum there is a dark spot about the size of half a crown, surrounded with a red areola; there is also a spotted eruption on the face; stools and urine since the 13th voided involuntarily.

To have applied over the sacrum the liniment of soap and opium.

In the evening the pulse had fallen to 100, but she was still stupid.

On the 17th she had two motions; the eye was clearer, but she did not answer questions or protrude the tongue. The face was of a more natural colour, but there was some degree of restlessness. No return of the fits since the morning of the 14th. She had a restless night, with frequent crying and vociferation. On the 18th the pupils were natural, but she cried frequently, with choking sensation in the chest and peculiar motion of the right arm. No stool since yesterday morning.

To take two compound colocynth pills at the hour of rest, and one ounce of the antimonial solution, if vociferation continue.

She remained much in the same state on the 19th and 20th. She had a quieter night on the 21st, but without sleep. She still, however, cried hysterically, and seemed to suffer pain from the blistered surface on the head, and also at the sacrum, where there is now a distinct slough, with an ulcerated margin. The pulse on the 21st was 100. There had been no return of the fits, but she was still stupid; not answering questions. There was no rigidity of the limbs; eyes suffused; no motion since the 19th.

A croton oil pill was ordered, with one drop of the oil; and a cataplasm was applied to the slough on the sacrum.

When seen in the evening the pill had not been given as one loose motion had taken place; the pulse was then 112. The skin and scalp were hot.

Cold applications were recommended to be applied to the head.

At this time it was my impression, that besides the epileptic, she was also suffering under a hysterical tendency, and that she was affected with continued fever there could be little doubt. I therefore mentioned her case to Dr Craigie, who most kindly offered to accompany me in my next visit. We accordingly went together on the 22d, and the following was her state at that time.

She had had no sleep for two nights, but she was quieter than she had been; the pulse was 134; the respiration 32, with some tracheal rattle. The eruption was still present on the face, and also on the arms, hands, and even fingers, but did not exist upon the chest. It consisted of small elevated spots. The eyes were suffused. When asked to put out the tongue she said she could not, being the first words she had spoken since the 13th. There was considerable rigidity of the hands, wrists, and elbows. The bowels had not been moved.

Dr Craigie confirmed me in my opinion, that she was labouring under fever, but also thought, from the state of the respiration, that there was some affection of the chest, although she never had any pectoral symptoms, except perhaps slight cough, which, however, was never more than often accompanies the more mild forms of fever. Dr Craigie therefore recommended one blister to be applied to the chest and another to the head, and one ounce of turpentine with half an ounce of mucilage to be given by the mouth.

In the evening when I saw her the pulse was 128; the superior extremites were still rigid; no motion or vomiting had taken place from the turpentine draught.

An enema, with two ounces of turpentine, was therefore recommended, and the feet to be bathed in warm water at bedtime.

On the 23d the pulse was 120. Two motions had taken place from the enema; she was otherwise much in the same state; the rigidity of the arms continuing. The blister on the head rose well; that on the chest but imperfectly.

At this time, from her low and weakened state, I thought it necessary to allow her a little wine, which she took with some arrow root, and seemed to relish it.

In the evening the pulse had risen to 136, but was feeble. There was, however, less rigidity of the limbs. The eyes were suffused, and there was still some eruption visible.

The turpentine enema, and warm foot bath were again repeated, and four ounces of white wine were allowed.

Two motions took place from the enema. Next day the pulse was 124, feeble, but not small. The respiration was 44, with less tracheal rattle. The arms were still rigid, and she was quite incoherent. She had taken three glasses and a half of white wine between eight o'clock of the night before and ten that morning.

A blister was applied to the chest, and the wine was directed to be continued. In the evening she was much in the same low state, and was with difficulty roused.

She was ordered a glass of wine every hour, and sinapisms were applied to the calves of the legs.

On the 25th and succeeding days the following were the reports.

25th. Between 7 o'clock last night and 10 this morning she had a bottle of port wine. The pulse is now 104, of better strength; lips and teeth covered with dark coloured sordes for the last few days. Feet rather cold; features collapsed and sunken; has been rolling the head upon the pillow a little; rigidity of arms gone; one motion.

The feet to be kept warm. Wine to be continued.

Vespere.—Has had a good deal of crying and vociferation during the last hour, with peculiar motions of the left arm; tossing and rolling of the head; but is now in the same low state in which she was previously. Pulse 120, small; feet and hands cold; no return of the fits.

A blister to be applied over the *occipito-cervical* region. A domestic enema with two ounces of oil of turpentine to be administered. The sinapised foot-bath to be employed. Wine to be continued.

26th. Two motions, and consequently enema very improperly not given. Pulse 112; extremities warm; some diaphoresis. When asked to protrude the tongue, she said "No, I wont." She has had half a bottle of wine from 8 P. M. yesterday till 10 A. M. this morning. Countenance improved; blister rose well.

Wine to be continued.

27th. Expression of countenance and eye clearer; pulse 120; had a restless night, with some crying; slough on the sacrum separating under the use of poultices. Some rigidity of the arms *to-day.* She is now very weak and low; respiration 44.

Wine to be continued.

28th. Much in the same feeble and exhausted state; countenance cadaverous looking; eyes wild looking, fixed, and glaring; no motion; pulse from 130 to 134.

Though with little hopes of effecting any beneficial change, it was believed right to exhibit the carbonate of ammonia under the circumstances now mentioned; and the following mixture was directed to be prepared.

Carbonate of ammonia, one drachm; powder of myrrh, half a drachm; aromatic electuary, half an ounce. To be dissolved and suspended in six ounces of water.

Half an ounce of the mixture to be given every third or fourth hour.

A domestic enema to be administered.

The wine was continued.

On the 29th, at seven P. M., she appeared to be very rapidly sinking; and at half past eleven she expired. She did not, however, die comatose, but sunk gradually.

Leave to examine the body having been granted by the friends, this was done two days afterwards; and Dr Craigie, who was present, took the following report of the morbid appearances.

"*Head.*—The inner surface of the cranium, on each side of the sagittal suture, and immediately anterior to the coronal, presented a rough elevated surface, with something of a honey-comb structure, especially on the right side; and on the outside of this a number of minute holes in the inner table of the skull, in which the *dura mater* had been firmly inserted. About three inches behind this, and one inch on each side of the sagittal suture, was a deep depression in the internal table of the skull, which corresponded to elevations in the *dura mater*. These elevations had the appearance of fungous excrescences, which had been lodged in the holes before mentioned.

"The veins of the hemispheres of both sides of the brain were distended with firm clots of blood, which, when the vessels were cut open, seemed mostly solid, but contained also some grumous blood, and at other parts they were filled with solid pale-coloured lymph.

"Under the arachnoid membrane, also, was effused a considerable quantity of serum, of a pale opaque colour, in whitish opaque patches. On the upper surface of the left side the vessels were also distended with clotted blood; and there was the same effusion of serum and lymph in the subarachnoid tissue of that side.

"The ventricles contained from four to five drachms of serous fluid, the greater part being on the right side.

"The parts forming the walls of the ventricles were healthy, except the *septum lucidum*, which was softened and attenuated, but not broken down.

"At the anterior extremity of the right hemisphere, about half an inch from the median fissure, and attached to the arachnoid and *pia mater*, was a small irregular scale of bony matter.

"Some serous fluid was found in the arachnoid membrane, at the base of the brain. The whole of the brain was healthy, and not unusually vascular.

"The whole of the longitudinal sinus, from the anterior to the posterior extremity, was filled to distension with a solid firm clot of blood, moulded to the form of the vessel adhering to it, and in part presenting the appearance of lymph; and in various parts the appearance of solid lymph, containing in its interior a quantity of semifluid bloody-coloured matter, like purulent matter. This coagulated blood also extended into the *torcular Herophili.*

"The Pituitary gland was large, red, and firm, and presented beneath it a cavity, holding a firm, amber-like, semitransparent substance, as large as a small bean, which, when taken out, sank in water.

"The *sella Turcica* was unusually deep and excavated; and the posterior clinoid processes broke off with very slight pressure, and were then found to be very much attenuated.

"*Chest.*—Upon cutting through the cartilage of the second rib, on the left side, a quantity of air rushed out; and it was found when the chest was opened, that the whole of the left lung was collapsed, and a pleuritic exudation was found effused over its lower and posterior part, while a small ash-coloured aperture was visible in the middle of the lower lobe.

"The lung was entirely removed and placed under water; and when air was sent into the windpipe, it was found to escape by a minute aperture. A section was made through this, and it was found to consist of a small ash-coloured cavity, emitting a gangrenous odour.

"At another part near the lower part of the upper lobe was a similar softened spot, reddish coloured, but not excavated.

"The right lung was pretty sound, except at its inferior lobe, where, near its posterior margin, was a large piece of pneumonic reddish coloured induration, and a small abscess near the surface. A similar patch was observed on the lower part of the upper lobe.

The uterus was unusually small, but its lining membrane appeared perfectly healthy.

In this very interesting case there are several circumstances well worthy of remark.

Judging from the history of the case, there could be little doubt that the patient laboured under epilepsy. Unfortunately I was never able to see her during a fit; but from the account given of the attacks by her friends, and her state immediately after them, I think I am fully warranted in pronouncing them to have been truly epileptic.

The manner in which the fits affected her was certainly peculiar; they were at first but slight, and occurred singly; and it is difficult to explain why, after continuing a year to trouble her, there should have been so long an interval as seven years without a single attack.

The species of epilepsy, to which it appears to me, the case related may with most propriety be referred, is the *epilepsia cerebralis.* The uterus certainly was small; its structure, however, was healthy, and menstruation had gone on regularly till within a year before she came into the hospital; and although at this time the fits increased in frequency, I cannot think that this was induced by the irregularity of the menstrual function. They more probably were connected with the morbid changes which must at that time have been going on within the cranium, and which were themselves most probably the cause of the uterine derangement, which in its turn might account for the hysterical symptoms which she all along manifested, as she frequently burst into tears without any cause, and was often affected with *globus hystericus.*

The opinion that this case belonged to the class *epilepsia cerebralis* and not *uterina,* is strongly supported by the appearances found within the head. I am fully aware that the proximate cause of epilepsy still remains in obscurity, notwithstanding all that has been said and written upon the subject. As, however, various morbid appearances have been found to occur in cases of epilepsy, and as these have been by some observers believed to be sufficient to produce the symptoms of the disease, and several of these lesions having occurred in the case under consideration, a few remarks on them may be proper.

The morbid lesions found in this case admit of division unde three heads.

Under the first head may be mentioned the effusion of seru under the arachnoid, into the ventricles, and at the base of th brain; the distension of the veins and sinuses with clotted blood lymph, and purulent matter; and the softened and attenuate state of the *septum lucidum.* All these may justly be considere as the results of inflammation, most likely of a chronic nature the commencement of which may with all probability be date from the time at which she became a patient in the hospital, a she never before that time suffered from the maniacal, I migh almost call them apoplectic paroxysms, with which she wa several times afterwards seized. The distension of the vessel with blood, lymph, and pus, was probably of more recent date and might, perhaps with more show of truth, be referred to inflammation taking its rise during the last apoplectic attack whic she had, and which began on the 12th of June 1841.

Under the second head may be arranged the singular honeycomb-like structure of the inner surface of the cranium, wit the minute holes and depressions on its surface; also the fungous excrescences and scale of bone attached to the *dura mate*

Under the third head may be placed the enlarged red an firm pituitary gland, the cavity beneath it containing the peculiar amber-like substance, and the remarkably attenuated an fragile state of the *sella Turcica* and posterior clinoid processe of the sphenoid bone.

All these appearances have been particularly dwelt on b writers on epilepsy as giving rise to the disease, but with wha truth has not as yet been satisfactorily established.

The pituitary gland was large, and when taken out of it situation it appeared to expand itself as if it had been confine within too narrow limits; it was red and firm, and apparentl in a state of subacute inflammation. The amber-like body presented exactly the appearance and consistence of a hard catara after it has been removed from the eye.

From a note which appears in Dr Cheyne's article on Epileps in the Cylopædia of Practical Medicine it would appear that i the treatise of the Wenzels on Epilepsy, translated by M. Breton, they lay great stress on appearances such as those above-me tioned as found in the pituitary gland and *sella Turcica,* in lesio of which, and of the pineal gland, they think the disease consis They mention the peculiar thinness of the sphenoid bone, clinoid processes, and *sella Turcica,* also different states of t pituitary gland. "In three cases it was very soft; in five fir compact, and of unnatural hardness; considerably enlarged, w an effusion of thick lymph between the two lobes. In seven prodigious size. In ten cases, at the point of union of the t lobes, there was a yellow, solid, friable matter, which might be raised in bits, &c." Numerous other morbid lesions are detailed. But enough has been said to show that there is certainly in many instances some connection between this disease and these morbid structures, and which the appearances found in the case before us tend to confirm.

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* Paris, 1811.

The disease found in the lungs was, I confess, wholly unlooked for on my part. There had been no symptoms sufficiently urgent to attract much notice to the chest. She had cough, but not more than is often present in ordinary cases of fever. The respiration was hurried at one time and natural at another, and this seems to be sufficiently well accounted for by her hysterical tendency. She had no expectoration, and the existence or non-existence of pain in the chest could not be determined, owing to her state of incoherence. But even had the extent of the morbid process going on in the chest been ascertained before death, I am not aware that anything more could have been done in her state to avert it. Two blisters were applied to the chest without benefit, and stimulants were carefully withheld, till, as a last resource, they became absolutely necessary. The intensity of the head symptoms, and the fever which, from the existence of the eruptions, the slough on the sacrum, the dark sordes on the teeth, and other symptoms, seemed distinctly to have been of the typhoid character, had completely masked the pectoral complaints.

The left lung was much compressed and reduced in size, and adhered closely to the spine. The perforation through which the air had escaped into the cavity of the pleura giving rise to pneumothorax was caused by the rupture of a gangrenous ulcer about the size of a small hazelnut, round which the pulmonary tissue was hard and condensed. In short, it was exactly similar to the gangrenous ulcers described by Dr Craigie in the 148th Number of this Journal, as having occurred in the last two cases related in his paper on Gangrene of the Lungs, the last of which I had an opportunity of seeing. A similar softened spot existed in the upper lobe, but it was not so far advanced, and had not become excavated. All this extent of disease was obscured, and rendered completely latent by the severity of the head symptoms, to which my attention was almost entirely directed.

DUNOON, ARGYLLSHIRE, *Nov.* 1851.

ART. III.—*Notice of an Instance of* MOLLUSCUM CHRONICUM. By HENRY NEBEL, M.D., Practitioner of Medicine and Midwifery at Heidelberg. In a Letter to Dr Craigie.

DEAR SIR,—It is very likely that you do not remember me any more; but I have not forgotten your kindness to me, when in August 1837, introduced to you by Dr W. Thomson, I had the pleasure to walk round with you repeatedly in the Royal Infirmary, where you also favoured me with your Clinical Report of the fever ward. Hence I take the liberty to write you a few lines concerning your interesting account of an instance of Molluscum Chronicum, published in the Edinburgh Medical and Surgical Journal, January 1851, which I have read with a double interest, as I happened to observe a similar case of this rare disease about ten years ago.

As your article contains, indeed, a monography of this disease, in which all cases are recorded, that ever have come to public notice, I wonder you did not remember the case observed by Willan, and described as Molluscum Pendulum by Dr Bateman, who represented the same in his "Delineations of Cutaneous Diseases," plate lx., fig. 3.* Bateman's work appeared in 1817. Your case came under observation in the summer of 1818, and was read by Dr Bateman, as you mention, to the Medico-Chirurgical Society in June 1819. The circumstance that neither you nor Dr B. himself said anything of Willan's case of Molluscum Pendulum, leads me almost to the idea that you take this as a different disease, though the delineation of it shows a remarkable likeness, at least with the two tumours of the palpebrae of James Roy; and though (page 113 of your article) very rightly, as it seems, only two different species of Molluscum have been distinguished, viz., Molluscum Diuturnum, and Molluscum Contagiosum.

Regarding the case which I happened to observe, I have made the following sketch at the time at which it came under my observation.

On the 13th of November 1841 I was called to the wife of Jacob K., at Handschurhsheim, a village two miles from Heidelberg), who had a fit of apoplexy, and was near the end of her pregnancy. Having gone on the field to work, she did not return home, though it was already late. Her husband, alarmed hereby, went thither to see what had become of her, and found her senseless, lying on the ground, where she had probably lain already for several hours. As he could not bring her away, he returned home to fetch a cart, in consequence of which at least another hour elapsed, till at last the patient was brought home and put to bed. By degrees she returned to life, but was unable to speak, and her left side remained paralysed. After a few days she began to stammer a little, and appeared to be pretty well in her right senses. Labour then came on, and the patient was delivered about six weeks before the term, of a healthy boy. Parturition went on naturally, and without particular difficulty. The uterus contracted itself afterwards completely, and no considerable hemorrhage took place. The patient, nevertheless, died the following day; not, as it appeared, in consequence of the delivery; but probably this had been accelerated by the approaching death, as we often see that nature, before the death of the mother, expels the child in order to save it.

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* It is figure 1st in which this is delineated.—D. C.

The *post mortem* examination showed a blood-coagulum of the size of the fist in the right lateral ventricle of the brain, which by this was exceedingly distended. The serum was already absorbed.

The circumstance in this case most interesting to me was a very rare cutaneous disease, MOLLUSCUM, with which the whole body was covered. The wart-like appendices varied in size from that of a small pea to that of a hazelnut. Some few ones were still larger. Most of them had the colour of the skin. In some places they were of a bluish-red colour. No member of the family of the deceased ever had a similar disease. The disease of our patient first began nine years ago, when she was in the family way. At that time, however, the appendices are said to have been very small. After her delivery they had diminished a little in size, and particularly in turgescence; but with the second pregnancy they enlarged and became more numerous. After the delivery they diminished again, but became no more so small as after the first child-bed. In a similar way things went on in the subsequent pregnancies, until the tumours became at last as large and far extended as I saw them. At most parts of the body, particularly on the trunk, they were so dense, that there was scarcely any where a spot of a square inch without such tubercles. At the side of the nipples there hang three or four appendices bigger than the nipple itself, and of so similar a structure and (brown) colour, that they could have easily been taken for as many nipples.

It was striking how much those appendices, so turgid before, relaxed after the delivery.

The examination of those appendices made after the death of the patient, showed that all the constituent parts of the *cutis* had participated in their formation. The larger ones contained a reddish mass of a similar, though somewhat firmer texture, as the skin of corpses with anasarca; though, even by hard pressing, no fluid could be extorted from them.

A piece of the skin from the breast, of the size of a square foot, covered with those appendices, I have taken away at the dissection and given to the Heidelberg Pathological Museum, where it is preserved.

As to the situation of the tubercles in my case, they were most numerous on the trunk, though there was no part of the surface without them. As to their form, they did not show so much variety as in your case; all of them being spherical or spheroidal, and more or less pendulous, the smaller ones similar to those represented as *molluscum contagiosum* by Dr Bateman, Fasciculus xi., plate 61. Many of them exceeded in size those represented as *molluscum pendulum* by Dr Bateman, plate 61, fig. 3, to which they also presented a likeness in shape.

I have not published my case, because, isolated as it was, I thought it would not be interesting enough; whereas, ranged along with similar ones, it could amplify the knowledge of this disease: and as the Edinburgh Medical and Surgical Journal contains your comprehensive article about this subject, I think a short sketch of it would perhaps not be unqualified to be published in the same Journal. If you are of the same opinion dear Sir, I leave it to your own judgment to give from my account what you think proper.

Believe me, DEAR SIR, yours very faithfully,

HENRY NEBEL, M.D.

Heidelberg, 10*th October* 1851.

Postscript to the foregoing Notice, by Dr Craigie.

The instance of MOLLUSKUM CHRONICUM, seen and noticed by Dr Willan, was mentioned in the original Memoir which wa sent to the Medico-Chirurgical Society, in the latter part of the year 1818. Dr Bateman, however, in whose hands the paper was placed, removed the passage referring to it, and substituted the sentence, beginning "Dr Bateman, to whom cutaneous nosography," and ending "with descriptive details."

As I felt that I had no title to modify or alter the paper from the form in which it was read to the Medico-Chirurgical Society in June 1819, this passage, on the responsibility of Dr Bateman was allowed to stand as he made it. This is the only explanation which I have it in my power to offer, for the omission which has been remarked by my friend, Dr Nebel of Heidelberg.

With regard to any reasons which may have influenced D Bateman, either in altering the paragraph in the original memoir or in giving to the council of the Medico-Chirurgical Society th opinion that it was unnecessary to publish the paper in th Transactions, I never permitted myself to think in any way. was young in the profession, without experience in matters of th kind, and I was entirely unknown. Dr Bateman was considerably my senior; he was regarded both by myself and the few friends whom I then had, to be a supreme authority in all matter relating to Cutaneous Diseases, and on many other points; and felt that to whatever opinion he gave, it was at once my duty and my interest to submit. Whether in this I was right or not it is now of no consequence to inquire.

While I am now explaining the cause of an alleged omission it may be proper to mention what may be regarded as another omission. In May 1841 Mr Frederick Hall Thomson published in the Lancet a notice of a case, which he refers to the head MOLLUSKUM CHRONICUM. In this instance the skin presented many tumours or spherical bodies varying in size; and on the thigh was a tumour which Mr Thomson regards as medullary. It is not perfectly clear to me that this is an example of genuine MOLLUSKUM; but as I had no means of knowing any thing about it except from the printed account, I thought that it was most proper to say nothing of its nature. Though the diagnosis of MOLLUSKUM is not in general difficult, yet it appears to me that to this head have been referred several examples of disease, which either doubtfully belong to it, or are altogether different. In my former paper I found it necessary to say regarding one of the cases given by M. Jacobovics, that it did not appear to belong to the same head as the MOLLUSKUM CHRONICUM, but was either a different, perhaps anomalous eruption, or was the effect of mercurial irritation. Another case which he has given is evidently an instance of tubercular cancer of the skin.*

That these remarks upon the necessity of caution and discrimination, in referring to the head of MOLLUSCUM, cases of tubercular disease of the skin are not uncalled for, will, I think, be rendered manifest by the following facts.

In the last published Number of the American Journal of the Medical Sciences, Dr Henry H. Smith of Pennsylvania reeords under the title of MOLLUSKUM developed by injury, an instance in which the skin of the left arm in a married female, aged fifty-five years, became covered in consequence, it is stated, of injury, by numerous small bodies or tumours, first about the size of peas, afterwards a little larger, and finally like small shot or bullets. The mass was first removed by the escharotic effects of the chloride of zink. The tumours returned in the cicatrix; and the arm was removed by amputation. Afterwards the stump and arm were attacked by them; and eventually the skin of the head, trunk, and other parts became occupied by these bodies. Dr Smith afterwards allows that this is medullary sarkoma; and it is difficult to see to what other morbid formation it can be referred. It seems, therefore, not accordant with correct diagnosis, or correct pathological principles, to apply to this disease the name of MOLLUSKUM. Yet such is the designation of this case. If it teaches nothing else, it shows the confused and inaccurate notions which prevail upon this subject.†

In making these observations, nevertheless, I wish it to be understood, that though I am desirous to fix upon just and irrefragable grounds, the character of the CHRONIC MOLLUSKUM, and avoid the error of referring to it every unknown and anomalous appearance of tubercles on the skin, I am perfectly willing to allow to others who have witnessed these cases and carefully examined them, the responsibility of classing them under the head of MOLLUSKUM, if they see cause sufficient and satisfactory for such course.

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* Edinburgh Medical and Surgical Journal, Volume lxxv., page 119.

† Case of Molluskum developed by an Injury, and presenting under the Microscope the Character of Medullary Cancer. By Henry H. Smith, M.D., Assistant Lecturer on Demonstrative Surgery in the University of Pennsylvania. American Journal of the Medical Sciences. No. xliv. New Series. October 1851. Article vii., p. 396.

I may observe, however, that so far as correct observation hitherto goes, it has not been proved, either that the CHRONIC MOLLUSKUM is connected with Medullary Carcinoma or Tubercular Cancer of the skin, or partakes in any way of those tubercular eruptions which occasionally in strumous and bad habits of body succeed to the use of mercury.

ART. IV.—*Analytical Inquiries on the Physiology and Pathology of the Spleno-Hepatic Apparatus.* By Dr J. H. S. BEAU,* Physician to the Hospital Saint-Antony, Assistant to the Medical Faculty of Paris. (Archives Generales de Medecine, January 1851. Quatrieme Serie, Tome xxv.)

UNDER the name of Spleno-hepatic Apparatus, I include the Spleen, the Liver, and the Portal Vein, which forms the communication between these two organs, and which places them in relation with the infra-diaphragmatic portion of the Digestive Canal.

I shall consider the Liver, the Portal Vein, and the Spleen, first in the Physiological or normal state, and afterwards in the Pathological or diseased state.

PHYSIOLOGICAL PART.

In order to study usefully the functions of the Liver, the Portal Vein, and the Spleen, it is indispensable to examine the subject in a historical manner. I shall, therefore, state the principal opinions that have prevailed among physicians, beginning with Galen, the first who treated this physiological question with the importance which it deserves.

The following passage from Galen shows what was the opinion of the ancients and of himself on the subject of the *Vena Portæ.* —"But as to these veins (the Mesaraic), as servants in cities, convey into the common store-house of the city the corn cleansed in the barn, to be prepared, and immediately to be rendered fit for the purpose of nourishment, in like manner, the food previously elaborated in the stomach, these veins convey to a certain place of digestion common to the whole animal body, which we name the liver. But the entrance into this place, divided into many narrow paths, is one which some ancient person, skilled, I believe, in nature, called PORTÆ (Πυλαι). This name has remained from that time to the present day. This name, further, Hippocrates himself, and, along with him, the whole family of the Asclepiadæ, adopted, praising the wisdom of that ancient philosopher, who compared to the civil distribution that which takes place in the animal body."*

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* At the meeting of the Academy of Medicine on the 14th of March 1850, M. Beau read to the Academy a Memoir bearing the title of *Pathogenic* Considerations on the Liver. This Memoir contained some parts of the present, among others, the history of the functions of the liver, and the mode in which the *ingesta* act in the production of hepatic disorders. The principal ideas of the present Memoir have been deposited at the Institute in 1846.

Thus Galen compares the mesaraic veins to the porters and carriers, who, in a well-governed city, convey the corn, previously cleansed, into a common store-house, in order that thence it may be distributed for the food of the inhabitants.

In the same manner, these veins convey the alimentary matter which has been prepared in the stomach, into a place of second elaboration, which is the liver. Among all the narrow entrances of this organ, the *Porta* he regards as the one in which all these are united.

In the following paragraph from the chapter entitled *de Usu Hepatis*, Galen returns to this action of the liver and the Mesaraic veins; but he adds a new circumstance, namely, that alimentary articles before being subjected to the action of the liver, have undergone in the mesaraic veins a previous modification, by which they are coarsely assimilated to the nature of the blood into which they are conveyed. "The liver itself," he observes, "after it has received from its servants that nutriment in a prepared state, and presenting a rude and obscure resemblance to the blood, effects in it the final preparation to the formation of perfect blood."† Thus according to Galen the liver effects in the alimentary matters those final changes which fit them to assume the characters and perform the duty of blood.

Again according to Galen, the liver cannot convert the aliments into fluid blood, without eliminating certain impure and excrementitial matters which are found in alimentary substances. These matters are of two sorts; one light and yellow is received by the gall bladder; the other thick and turbid is conveyed into the spleen.‡ According to this view the gall bladder is the receptacle of the yellow bile; the spleen is the spongy receptacle of black bile, (*atra bilis*), or *melancholia;* and by this spongy organization, the spleen ought like a sponge to absorb the melancholic fluids of the liver.§

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* De usu Partium, p. 135. Apud Juntas, 1565.

† Ipsum autem hepar, postquam id nutrimentum acceperit a famulis jam praeparatum et veluti rudem, quendam delineationem obscuramque speciem sanguinis referens, inducit ei postremum ornatum ad sanguinis exacti generationem.

‡ Siquidem vesicam quae leve et flavum superfluum receptura erat, natura imposuit hepati; splenem vero qui crassum et limosum attracturus erat. Ibid. p. 136. B.

§ Facultatem habet splen ad seipsum trahendi succos melancholicos, rarum ad modum ac laxum instar spongiæ cujusdam, ut succos crassos facile et attrahat et excipiat. Ibid. p. 140. B. *De Lienis Usu.*

The liver, which, as has been seen, is the principal organ of sanguification, Galen calls the instrument of *hæmatosis.**

This theory of Galen prevailed for a very long time among physicians. It was successively adopted by the Arabians, the Arabists, and particularly by the anatomists of the period usually called the revival of literature, among whom Vesalius must be placed.

Different was the situation of the hypothesis when Asellius in 1622 discovered the chyliferous vessels. The theory of Galen was shaken by this discovery; it was considerably modified by it, but it was not overthrown. In point of fact, Asellius demonstrated that the chyle was absorbed by the chyliferous vessels, instead of being taken up by the rootlets of the *vena Portæ*, as had been previously thought; but he at the same time adopted the idea that the chyliferous vessels conveyed the chyle into the liver. So much was it still desired to preserve to this organ the function of preparing the blood.

This relic of the ancient physiology conceded to the liver could not be long continued. This gland was in truth completely deprived of its hæmatosic functions in consequence of the discovery of the receptacle of chyle by Pecquet. This anatomist proved in opposition to Asellius, that the chyliferous vessels proceeded not to the liver, but to the receptacle which he had discovered, and that consequently the chyle, instead of being conveyed to the liver, was carried first to the *Receptaculum*, then into the thoracic duct, and finally was poured into the subclavian vein.

Riolan, faithful to his principle of taking the side of the authority of the ancients against the discoveries of his contemporaries, failed not to attack the doctrine of Pecquet as keenly as he had attacked that of Harvey on the Circulation; but he met with a rough adversary in the person of Thomas Bartholin, who had to defend his own share of the honour in the discovery of the thoracic duct and of the lymphatic vessels. This last anatomist published on this occasion a series of writings, the titles of which show how completely the alleged functions of the liver were annihilated. One was entitled, *Vasa Lymphatica nuper in animantibus inventa, et hepatis exequiæ:* Paris, 1653. The *exequiæ hepatis*, or obsequies of the liver, were in truth as complete as possible; for the author has not recoiled from the idea of making on the extinction of the liver as a hematopoic organ, an epitaph, not void of humour.

The new doctrine maintained by Bartholin was adopted almost unanimously. But the ideas of Galen were reproduced anew by Louis De Bils, a Dutch gentleman, who had made himself known by acts of charlatanism and cupidity, in reference to a secret which e pretended to possess as to a method of embalming dead odies. De Bils published in 1659, at Rotterdam, a Dissertaion, in which he maintained that the chyle passes in great part nto the liver by means of the *vena portæ*, and that this gland is he organ which prepares the blood.[*]

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* In hepate quod supponitur venarum principium esse, primum sanguificationis quam haematosin vocat instrumentum. Ibid. p. 138. D.

The publication of this performance raised a universal opposition. "Against this heresy," says Haller, "almost the whole anatomical world arose in resistance."[†] Bartholin published a refutation of the experiments of De Bils, in the title-page of which, taking it for granted that the liver was scientifically dead, he represents the resurrection of this organ to be a matter of great difficulty.

Thus terminated this controversy. The honour remained with Bartholin; that is to say, the liver was decidedly interred, as this anatomist wished. It ceased to be regarded as a sanguifying organ, as an organ concerned in the function of Haematosis. This function was progressively assigned to the lung, which, in consequence of the beautiful discovery of Harvey, was considered for the first time, as an organ entrusted with the function of preparing and modifying in a powerful manner the circulating fluid.

Changes also took place as to the bile; but this fluid acquired increased importance. It was no longer regarded as an excrementitial fluid, but as a liquid separated from the blood by the glandular and secreting action of the liver for important purposes, and as one indispensable to digestion.

The organ which suffered still more than the liver by this revolution in physiological doctrine, arising from the discovery of the chyliferous vessels, was the *Vena Portae*. Bartholin had indeed more reason to write the epitaph of the portal vein than that of the liver; for while the liver continued to exist as the organ which secrets the bile, the portal vein, deprived of its functions of absorption, no longer enjoyed any special value. Its terminal rootlets ramified on the one side in the alimentary canal, on the other in the substance of the liver, remained as a fact inexplicable and singularly exceptional in viewing the anatomy and physiology of the venous circulation.

Such were then the results of the revolution which took place in physiological science by the discovery of the lymphatic vessels;—the liver dispossessed of its functions of hæmatosis, and concerned only in the secretion of bile; the absorption of alimentary substances effected by the chyliferous vessels to the exclusion of the *vena portae*. These are the physiological notions which we find on this subject in all the authors of the last and the beginning of the present century;—in the works of Haller, Hunter, Bichat, and similar writers.

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[*] Bilsii Epistolica Dissertatio, qua verus hepatis circa, chylum et pariter ductus chyliferi hactenus dicti usus docetur. Rotterdami, 1659.

[†] Haller Bibliotheca Anatomica Bilsius. Bartholini Responsio de experimentis Bilsianis et difficili hepatis resurrectione. Hafniae, 1661.

During the last thirty years, however, a reaction has taken place in favour of the theory of Galen, and this reaction must be ascribed chiefly to the labours of M. Magendie. This able physiologist has proved, by the aid of decisive experiments, that the venous rootlets of the gastro-intestinal tube absorb the different liquids formed within this canal, with the exception of the chyle. The liquids absorbed by the veins of the stomach and the intestine are conveyed with the venous blood, which carries them into the trunk of the portal vein, and thence into the liver. The chyle, on the other hand, is taken up by the chyliferous vessels, and conveyed into the thoracic duct.

M. Magendie, nevertheless, does not carry the hypothesis so far as to recognize in the liver, like Galen, an action by which this organ assimilates to the blood the alimentary substances absorbed by the portal vein. He sees in this matter only a mechanical function in the liver, which, traversed, so to speak, like a sort of filter, by the portal blood, would have for its object to mingle intimately with this blood the alimentary matters which it carries.*

Subsequent to Magendie, Messrs Tiedemann and Gmelin proved, by numerous experiments, the absorbing faculty of the portal vein, and the passage of the matters absorbed through the liver; but as to the peculiar action of the liver, they proceed further than M. Magendie; for they allow that this organ exercises upon the aliments absorbed a change which causes them to approach to the composition of the blood.†

M. Blondlot of Nancy admits, like the physiologists of Heidelberg, that the portal vein absorbs and conveys alimentary matters different from the chyle; further, he endeavours to show exactly that the liver acts upon the matters absorbed by the portal vein, by assimilating them to the blood, and by eliminating those parts of these matters which cannot be transformed into constituent elements of the organism. These eliminated matters form the bile. M. Blondlot thinks, like Galen, that the bile is the excrement of the matters absorbed by the portal vein, and that it fulfils no essential function in the process of digestion.‡

Lastly, M. Bernard has made several discoveries in the physiological inquiry now under consideration. M. Bernard has shown, in a striking manner, the following points; 1*st*, that the chyle is nothing else than the fatty matter of the alimentary articles converted into an emulsion by the pancreatic juice. The chyle is absorbed by the chyliferous vessels, and proceeds to be poured by the receptacle of Pecquet and the thoracic duct into the left subclavian vein, while all the other substances absorbable join the intestine, enter the rootlets of the portal vein, and are carried to the liver. *2d*, The experiments of M. Bernard show that, at the same time that the liver secretes bile by the biliary vessels, it produces in the rootlets of the infra-hepatic veins an abundant saccharine matter, which is conveyed, with the blood of these veins, into the great current of the *vena cava*, and thence into the lung, where it is destroyed by contact with the atmospherical air. The liver further secretes fat, and produces fibrine, which, with the sugar now mentioned, is conveyed by the blood of the infra-hepatic veins into the general circulation.

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* Precis Elementaire de Physiologie, Tome ii., pp. 258–260. Paris, 1833.

† Recherches sur la route, que prennent diverses substances pour passer de l'estomac, et de l'intestin dans le sang. Traduit de l'Allemand par Heller. Paris, 1821.

‡ Essai sur les Functions du Foie. Par M. Blondlot. Paris, 1846.

According to M. Bernard, the sugar, the fat, and the fibrine, maintain the blood in the conditions necessary to the continuance of life. In this point of view, the liver is therefore a powerful organ of hematosis or sanguification. As to the bile, that fluid acts by ridding the blood of materials unfit for nutrition, or in excessive proportion; for instance, carbon. But though the bile be in this manner a sort of excrementative residue, M. Bernard thinks that it performs an important part in the function of digestion. Mingled with the gastric juice and the pancreatic fluid, it forms the intestinal liquid, which dissolves vegetable matters, hydro-carbonated matters, and azotized articles. Further, the bile appears to possess antiseptic properties; it prevents fermentation, and withstands the development of those gaseous fluids which always result from the decomposition of azotized and non-azotized matters.

Such is the present state of Physiological knowledge as to what concerns the functions of the liver, and the absorption of the matters contained within the intestinal tube.

Some important developments, I think, ought to be added, in order to complete what we know upon the functions of the Portal Vein. In truth, the blood of the Portal Vein is not only the excipient or the vehicle of the matters absorbed from the intestine; every circumstance leads us to believe that it exerts also a particular action upon these matters during the transition which they undergo from the digestive tube to the liver.

Indeed physiologists are nearly unanimous in admitting, in the blood in general, the power of destroying and assimilating certain alimentary substances; for instance, glucose (Bernard, Mialhe), alkohol,[*] (Bouchardat, Sandras), &c. Now, the blood of the

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[*] We can scarcely believe that this is said seriously, and we feel it impossible to allow the statement to pass without expressing the opinion, that either there is a typographical error of Alkohol for Amidon, or that the statement that alkohol is decomposed by the blood, is altogether erroneous and unfounded. Certain it is, that, if very minute quantities of alkohol seem occasionally to be destroyed, considerable quantities, according to all the experiments and observations made in England, are not. Alkohol has been repeatedly obtained by distillation from the tissues of animals poisoned by it, and of men and women who have died under its use. If it were *amidon*, starch, the statement is intelligible enough. That substance, like

Portal Vein being the first which is found in contact with these substances on their emergence from the intestinal tube, is consequently the first which ought to exercise upon them this destruction which assimilates them with the organism.

I add meanwhile, that if the portal blood cannot fail to assimilate the substances which it receives on leaving the intestinal tube, we may hence conclude that the special function of the portal blood is to produce this assimilation.

Certain anatomical circumstances appear to demonstrate the fact, that this induction is well founded.

At first it may be observed that every circumstance concurs to prolong the stay of alimentary matters within the system of the Portal Vein, which is divided at its two extremities into a great number of branches, and capillary vessels. Further, this vein is not provided with valves; which permits the reflux of the portal blood during the efforts and the acts of respiration; and which finally retards the passage of alimentary matters, and of the blood which contains them.

Another condition very important for the assimilation of alimentary matters within the Portal Vein, is the circumstance that the quantity of the assimilating blood is much more considerable than the quantity of the matters to be assimilated. This condition appears to me to be well established.

Let us suppose indeed that matters assimilated are absorbed solely by the venous rootlets of the upper part of the ileum; these matters would be found within the absorbing rootlets with too small a quantity of blood to admit of their undergoing any perceptible modification. But in proportion as these matters shall advance towards the liver, they will come gradually to be mingled with the blood proceeding from the branches of the stomach, of the large intestine, and other parts which have absorbed nothing. This blood being pure from all reaction and increasing in quantity, in consequence of the progressively increasing branches by which it is furnished, will cause to be induced gradually in the alimentary matters the modification which is requisite to assimilate them.

There are then, as we see, in the gastro-intestinal tube contemplated during the progress of digestion, absorbing venous branches and assimilating venous branches. The absorbing venous branches are alternately in those parts where there are matters to be absorbed; sometimes in the small intestine, sometimes in the stomach, and sometimes in other parts; and during this time, the other venous branches of the gastro-intestinal tube are performing the

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glucose, dextrine and sugar, is destroyed or decomposed, or probably transformed into fat, or even the carbon of the animal tissues. There is, on the contrary, no reason to believe that alcohol undergoes either destruction or transformation under the action either of the fluids, the solids, or the influence of air upon them.

important office of assimilating branches, that is, they are conveying virgin blood, which is to add its powerful action to that of the blood of the absorbing branches, in order to aid in the assimilation of the matters absorbed.

We have now proceeded on the supposition that the absorption of alimentary matters is accomplished only by one single point of the gastro-intestinal tube; but we know that this is not always the case. Often indeed after the introduction of considerable quantities of food, the absorption of the assimilable matters is effected at a certain time of the process of digestion, by almost the whole system of the veins of the digestive canal, and consequently at that time the gastro-intestinal venous branches almost all perform the office of absorbing branches.

Now the blood which arrives from the stomach and intestine, and which thus transports the matters absorbed, being too inconsiderable, in proportion to the mass of these matters, would be in like reason, as has been already said, powerless to effect in them suitable assimilation. But for this deficiency nature has made provision, by utilising for the necessities of this assimilation the veins of the mesentery, of the omentum, of the pancreas, of the gall-bladder, and of the spleen, which convey by numerous affluent branches a large supply of assimilating blood upon the matters absorbed by the gastro-enteric veins.

Matters being understood in this manner, there is no longer anything incongruous in the functions apparently different of the original or convergent branches of the portal vein, of which some, namely, those of the digestive canal absorb, while others, namely those of the pancreas, the epiploons, the spleen, and similar organs do not absorb. Of all taken collectively, the object and purpose is to absorb and assimilate the alimentary matters. The first class of veins are at the same time absorbent and assimilant, while those of the second class are purely assimilant.

It is further necessary that there should be branches purely assimilant, because it is necessary that, how considerable soever be the quantity of matters absorbed, there should at all times be in the system of the Portal Vein a mass of blood still greater in amount, in order to transport and assimilate these matters. In order to meet this necessity, Nature doubtless wished that the veins of the mesentery, of the epiploons, of the pancreas, of the gall-bladder, and of the spleen, should contribute their contingent of dark coloured blood to the current of the Portal Vein, instead of pouring it into that of the *Vena Cava.*

It further results from the foregoing considerations, that before the formation of the trunk of the Portal Vein, the composition of the blood is homogeneous, because in certain branches it conveys alimentary matters, and in other branches it contains none at all.

But counting from the trunk, the mixture of blood and of alimentary products is complete, and the assimilation of these products may be continued as far as the liver, which thus receives, by the numerous terminal branches of the Portal Vein, blood identical and uniform in nature, in all the points of the hepatic parenchyma.

Since the function of the blood of the Portal Vein is to absorb the alimentary articles, and to destroy them during their passage from the digestive canal to the liver, it becomes requisite that the portal blood should be of a different nature from that of the *vena cava*. It would require to be less coagulable, more solvent, more liquid, so to speak, than the latter; and this is necessary, not only in order that this blood might induce a change in the substances ingested as food, but especially in order that the blood should not be altered by these substances.*

Now, this difference in nature between the blood of the two veins is a fact which cannot be called in doubt. M. Beclard, who has recently devoted himself to curious researches on this subject, has ascertained that the fibrine extracted by beating the blood of the portal vein (superior mesenteric vein), is not elastic, does not run into filaments, but into small masses. Left to itself, in open dry air, it is completely liquified in the course of some hours; while the fibrine extracted simultaneously from the arterial and the venous blood (that of the *vena cava*), and placed in the same circumstances, is, on the contrary, dried.†

As is seen, the deliquescence of the fibrine of the portal blood forms an essential difference between this blood and arterial and venous blood (that of the *vena cava*). But it would be requisite to know if this fibrine, thus deliquescent, owes not its exceptional nature to a large proportion of alkalis; if there is not in the Portal Vein a superabundance of oxygen necessary to burn the hydro-carbonated aliments; if, in short, this is not a peculiar agent, a species of diastase, which all at once thus changes the nature of the fibrine, and modifies the alimentary substances. These are uncertain points upon which it will be requisite to artive at some determinate inference, in order to give a precise view of those functions which present are merely indicated by simple indication.

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* M. Liebig thinks that the absorption of alkohol by the veins is impossible, because the contact of alkohol with the blood has the effect of coagulating this fluid. It is probable that he has drawn this conclusion from the coagulation of the blood of the *Vena Cava*, and has thus erroneously applied it to the blood of the *Vena Portæ*.

[On this it may be remarked that the absorption of pure alkohol by the veins in quantity is either impossible, or so difficult, that it may be viewed as impossible. Alkohol, when absorbed by the veins, requires to be much diluted, because, in the pure form, not only does it induce a species of coagulation, but it crispates, hardens, and corrugates the venous tissues to a degree so great as to prevent the veins from absorbing. It is, however, taken up by endosmosis after a certain time.—Ed.]

† Recherches Experimentales sur les Fonctions de la Rate et sur celles de la *Veine Porte*, Archives Generales de Medecine, Octobre, Novembre, Decembre, 1848.

Induction, however, has given us here a solid foundation. I repeat, before proceeding further, the propositions which have opened to us the path of the considerations into which we have entered.

1. We recognize the fact, that the alimentary substances, which enter into the blood by means of absorption, undergo, from the action of the blood, a change which modifies them, destroys them, assimilates them.

2. The blood of the Portal Vein being, of all the forms of blood, the first which comes in contact with the aliments absorbed, is thenceforward that which has for its special function to modify, to destroy, to assimilate alimentary substances.

3. For this functional purpose of the Portal Vein, nature has made provision, by disposing its original branches in such a manner, that, while the one set carry the alimentary matters with the venous blood from the digestive canal, the others pour upon this mixture of blood and alimentary substances a large proportion of blood purely assimilating.

4. The blood of the Portal Vein being less coagulable, more liquid, more solvent, than the other kinds of blood, is perfectly adapted to its assimilating functions.

Thus, therefore, as has been above said, the blood of the Portal Vein is not only the excipient, the vehicle which conveys the aliments from the digestive tube to the liver; it is, further, an agent, or true solvent, which, while carrying the alimentary substances, modifies, destroys, assimilates them.

The Portal Vein may therefore be viewed as a special reservoir, placed between the digestive tube and the vascular system, and partaking at once of the characters of both; a reservoir, in which is accomplished the first contact of alimentary substances and the blood, which is at once the last process of digestion and the commencement of sanguification.

Every circumstance induces us to admit that the peculiar action of the liver is directed upon the materials of the aliments, modified or assimilated by the Portal blood.

The recent discoveries of M. Bernard show that the products of the hepatic elaboration are, on the one hand, sugar, fat, fibrine, all of which are poured by the infra-hepatic veins into the current of the *vena cava;* and, on the other hand, the bile, which flows by its proper vessels into the gall-bladder, and into the duodenum. These products, the one of sanguification, the other of elimination, are the result of the action of the liver upon the materials of the aliments modified by the blood of the Portal Vein.

On the subject of fibrine, however, it is requisite to make one observation. We know that the principal character of the Portal blood consists in possessing a soft, imperfectly coagulable, and deliquescent fibrine; and as the fibrine of the blood, furnished by the infrahepatic veins, is perfectly coagulable, and not deliquescent, it necessarily follows, that this change in the nature of the fibrine has been effected by the proper action of the liver.

It is not necessary, then, to compare this re-constitution of the fibrine by the liver, to the formation of sugar and of fat, which are products of the alimentary process. M. Bernard, nevertheless, regards the soft fibrine of the portal blood as an alimentary product, as a modification of the fibrine of the aliments, which he denominates, with M. Mialhe, ALBUMINOSE;* and he admits that this ALBUMINOSE is transformed into fibrine by the liver, which thus produces fibrine, as it forms sugar and fat.†

I am quite disposed to recognize the modification, in consequence of which the fibrine of alimentary articles passes into the state of Albuminose, and thus modified, and in the form of soft fibrine passes into the portal system, in order to be changed by the liver into true fibrine. But I must remark, that it would be highly important to distinguish this ALBUMINOSE from the soft and deliquescent fibrine which characterises the portal blood in the normal condition, and which is found in this blood at periods when digestion is not proceeding, and in animals which do not eat fibrinous articles of food. It would be requisite, in short, to demonstrate precisely that there is in the liver a two-fold formation of fibrine; 1st, The transformation into vegetable fibrine of the deliquescent loose fibrine which is permanent in the portal blood; and, 2nd, The transformation into fibrine of the ALBUMINOSE, which is formed in a transitory manner in the portal blood, and which enters it at the moment at which the digestion of fibrinous aliments is proceeding.

Here, then, as I above said, have we completely returned, excepting the modifications as to precision, to the ideas entertained by Galen. At the present, we recognize in general terms the three following propositions.

1st, The Portal Vein absorbs the alimentary articles, and conveys them to the liver.

2d, The Liver is an organ of Sanguification and of *Hæmatosis;* it is the organ of alimentary hæmatosis, while the lung is the organ of aerial hæmatosis. As to the bile, that substance is an excrementitial residue arising from the elimination of matters unfit for hepatic sanguification, and fulfilling nevertheless important functions in the process of digestion.

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* M. Beclard, in his Analysis of the Blood of the Portal Vein, already referred to, ascertained that, during the process of digestion, this blood contains a large proportion of albumen. He thinks that this albumen is nothing else but the transformation of azotized substances, which, in this form, enter into the mass of blood. Was this Albumen or Albuminose?

† L'Union Medicale, 3 Septembre 1850.

3d. Everything leads us to admit, with Galen, that the substances ingested and absorbed before arriving at the liver undergo a primary modification or assimilation from the blood of the Portal Vein. We have quoted one passage from this author on this subject. Here is a second passage in which the same opinion is stated in very positive terms; "Venis quæ ad ventriculum et universa intestina pertinent, inest facultas quædam sanguinis effectrix, qua succum qui ex cibis distribuitur, venis in sanguinem mutare naturale est, prius quam is ad hepar perveniat." *

There is in the theory of Galen one point upon which modern physiologists are completely at variance with this author. It is that which relates to the origin and nature of the chyle.

It is the opinion of Galen that the entire nutritive part of the aliments is reduced in consequence of elaboration in the stomach to one single principle, always identical, a true succus or juice, as may be seen in the preceding passage. To this Galen applies the Greek denomination of chyle (χυλος), which means merely *Succus.* It is this fluid or *succus* which is conveyed by the Portal Vein to the liver; and in some passages Galen calls this last vessel the *Vein of the Chyle.*

This error of Galen upon the unity of the alimentary juice or chyle has prevailed among physiologists to the commencement of the present century. It was the cause of the violent discussions which took place on the subject of the liver at the period of the discovery of the lymphatic vessels. It was indeed very difficult, or rather impossible at that period to arrange the liver along with the chyliferous vessels; for from the moment that it was well established, that the alleged single alimentary principle, the chyle in short, traversed the chyliferous vessels in the form of a white milky fluid, in order to proceed by the thoracic duct into the subclavian vein, physiological reasoners must have inferred, that the nutritive or chylous portion did not penetrate into the blood by the Portal Vein and the Liver; and, consequently, they must have refused to ascribe to the liver the functions of Hæmatosis, which Galen had assigned to that organ.

By means, however, of the recent discoveries, and the popularized form in which they have been taught by M. Berard, it is possible to conciliate the adversaries of the preceding discussions; for upon the true nature of chyle, physiological opinion is perfectly fixed. It is known that the liquid which bears this name is nothing else than the fatty portion of the alimentary matters converted into an emulsion by the pancreatic liquid. All the other alimentary substances, drinks, and similar articles, are absorbed by the veins of the stomach and intestines, and enter by this channel into the liver. Thus, then, the fat traverses the chyliferous vessels and the thoracic duct, while all the ingesta not fatty proceed to the liver.* We shall afterwards see that this error upon the chyle, viewed as one single alimentary residue has also exercised a detrimental influence upon the pathology of the liver.

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* Galeni Opera De usu Partium; De usu Hepatis. 138 E.

In order to finish the physiological part of this Memoir, it remains to explain some considerations upon the spleen, upon which nothing has been said since the quotation which I have made from extracts from Galen.

We know that Galen regarded the spleen as an auxiliary organ of hepatic haematosis. According to this author, it served with the gall-bladder, to rid the liver of alimentary matters unfit for the purpose of sanguification. The impure and excrementitial matters were of two sorts. One light-yellow went to the gall-bladder; this was the yellow bile, or bile properly so named. The other thicker, more turbid, blackish, went to the spleen; this was in the mind of Galen, the black bile or melancholia. The spleen in consequence of its spongy consistence, could thus attract to itself and retain the atrabiliary excrements of the liver. "Facultatem habet Splen ad seipsim trahendi succos melancholicos, rarum ad modum ac laxam instar spongiæ cujusdam ut succos crassos facile et attrahat et excipiat."†

This doctrine of Galen on the uses of the Spleen had the force of law in the Schools so long as physicians ascribed to the liver the important functions of transforming the alimentary articles into blood, that is as late as to the middle of the seventeenth century. Subsequent, however, to the discovery of the chyliferous vessels and the receptacle of Pecquet, the liver was no longer believed to possess the functions of hæmatosis, and at the same time physicians ceased to recognize in the spleen the functions which Galen had assigned to it. From the period indeed, at which it was taught that the aliments did not pass by the Portal Vein and the Liver, it followed that there could no longer be in the liver impure and excrementitial matters for the spleen to attract to itself and retain.

As has been already said, hepatic hæmatosis being at this period regarded as impossible, the liver was regarded simply as the organ which secretes the bile; and the portal vein being no longer regarded as the organ of alimentary absorption, was little else but an explicable anomaly in the venous system. The same was the case with the spleen for which physiologists began to seek new functions. But they no longer adhered to the condition that its functions should be auxiliary to those of the liver; for as the significant union of the porto-hepato-splenic system was no longer admitted in physiological doctrine, the spleen became like the liver, and the portal vein, an individual existence completely isolated and unconnected.

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* Upon this statement the author subjoins a note remarkable at once for its simplicity and serviceableness to the argument. "This distinction," he says, "must not be taken in a strictly literal sense. It is probable that fat penetrates into the portal vein, and that aliments not fatty are absorbed by the chyliferous vessels.

[Assuredly, it is probable that such penetration takes place; and the probability is so great that it amounts to as great certainty as it is possible to obtain in physiological questions. In short, it appears that the statement in the text is altogether a figure of speech. Whether it is a necessary one in the argument or not, the author is the best judge.]

† Galeni Opera de Usu Hepatis, p. 140. B.

Before mentioning the principal functions which physologists from this period ascribed to the spleen, I ought to notice an alleged discovery made nearly at the same time as that of the chyliferous vessels, and which showed in the most positive manner that the spleen could not attract to its structure the pretended black bile. Father Honoré Fabri, a Jesuit, who was, as is known, one of the claimants to the honour of having discovered the circulating motion of the blood, first proved that there was in the splenic vein, a constant current, which proceeded from the spleen towards the liver. With the existence of such a current, the hypothesis of Galen upon the spleen was impossible; it was therefore completely and for ever relinquished.

The opinions which prevailed among physiologists upon the uses of the spleen, after the downfall of the doctrine of Galen, had no longer with a certain degree of necessity as object, as has been already stated, to reconcile its functions with those ascribed to the liver. In this spirit different speculatists considered it as the seat of laughter, as the abode of the sensitive soul, as the seat of the sexual appetite; some admitted that it furnishes the elements of the seminal fluid, and even of the cerumen of the ears. Some persons struck by its direct relations with the liver, thought that it secreted a fluid suitable for preparing and tempering the bile. One author represented it to be the organ in which were formed the red globules of the blood; another considered it as the channel for supplying to the stomach the blood, from which was secreted the gastric juice. Several writers have regarded as a *Diverticulum* or Reservoir for the blood, both during digestion and during running. This last opinion shall be afterwards examined. Lastly, quite recently M. Beclard, in a performance which we have quoted, aud the use of which I shall turn to further profit, has promulgated the opinion, that the globules of blood, which according to him are formed in the intestinal branches of the portal vein, are conversely destroyed in the spleen. I shall direct a little attention to the structure of the spleen, before stating the probable inferences, which analytical induction furnishes on the subject of its functions.

The structure of the spleen is peculiar, complex, and has not yet been determined with precision. It is constituted by two principal elements; 1st, a vascular areolar element; and 2dly, a glandular tissue.

1. The vascular areolar element communicates freely with the rootlets of the splenic vein, by means of which it is very easily injected. It is eminently dilatable and retractile; and this it is, consequently, which causes variations, so many and so suddenly occurring in the volume of the spleen.

2. Of the glandular element, which is less generally admitted than the preceding one, the character is an agglomeration of small reddish grains, quite similar to the granulations of the glandular organs. To render this glandular element apparent, it is requisite that the spleen be as much as possible returned upon itself by the retraction or shrinking of the vasculo-areolar element, as is observed in individuals who have not eaten food for a long time. In this case the granules of the glandular element are so closely approximated to each other, that when we tear the spleen we find in it an aspect very similar to that of the cortical substance of the kidney. Conversely, when the vasculo-areolar element is dilated and filled with blood, there is produced a proportional separation of the splenic granulations, which then may be so disseminated, and, as it were, lost in the parenchyma of the spleen, that this organ appears to be of a vascular and areolar structure, and not at all glandular.

Consequently, the circumstance which anatomically characterizes the spleen is a structure which is neither entirely vasculo-areolar, nor entirely glandular; but one which is half vasculo-areolar, and half glandular. The two elements, vascular and glandular, are combined together in such a manner, that when the one predominates, it is necessarily, and in the same proportion at the expense of the other, which is then diminished. With the predominance of the first element, the splenic organ is voluminous, blackish, and soft; when the second predominates, it is small, hard, and reddish.

As may be conjectured, these elements have been distinguished and described; but, as is often the case, the description of each has been given to the exclusion of the other; one class of anatomists maintaining that the spleen is entirely glandular, others that it is an organ solely vascular and erectile. These two opinions, nevertheless, it is requisite to reconcile, in order to make be recognized in the spleen a complex structure, which causes this organ, solitary in its kind, to belong at the same time to the order of glandular organs, and to the tribe of vasculo-areolar tissues.

We next pass to the functions of the spleen.

When we consider, that the spleen sends into the current of the Portal Vein the whole of the blood by which its own substance has been traversed, and that by means of this vein it readily communicates with the liver, it is impossible to resist the idea that the spleen is an organ, the functions of which cannot be separated from those of the Portal Vein, and consequently from those of the liver. It hence follows, that the Spleen, the Portal Vein, and the Liver, concur together to one common physiological operation; and, consequently, if we know the functions of the portal vein and of the liver, we may, proceeding from these as an initial point, and guiding our course by the experimental facts acquired by science, discover the indication of the functional part of the spleen in the collective operation to which it concurs.

Now, we know with a sufficient degree of precision, that the Portal Vein and the Liver are subservient to the sanguification of alimentary articles; the Spleen, consequently, also serves to contribute to this important process. It remains to be discovered in what manner it answers this purpose. We may state that we are about to find once more the triorganic sanguificator of Galen, with this considerable difference, however, that in the theory of Galen, an imaginary current was supposed to proceed from the liver to the spleen; while in the synthetical exposition which I am now to offer, it is indispensable to bear in remembrance the current which proceeds in the contrary direction from the spleen to the Portal Vein and the Liver.

We must first remind readers that the Portal Vein contributes to sanguification by absorbing alimentary substances, by making them undergo, while they are being transported into the liver, an assimilating modification, which precedes that more important one, by means of which the liver transforms them into sugar, fat, and fibrine. Consequently, as has been said, the portal vein absorbs, transports, and at the same time assimilates the alimentary articles.

I have already said, that in order that this primary assimilation of alimentary substances may be duly accomplished, it is requisite that the quantity of assimilating blood be much more considerable than that of the substances to be assimilated. For, were it otherwise, it might happen that the portal blood, instead of exerting a modifying power over the matters absorbed, might on the contrary be modified by them; as for example in the cases in which the *ingesta* are endowed with coagulating properties, such as certain acids, alkohol, and similar substances.

We have shown that nature has insured this condition of assimilation, by causing to be poured into the current of the portal vein all the venous blood which returns from the gall-bladder, from the pancreas, from the spleen, &c.; so that the amount of venous blood brought by the cystic, pancreatic, and splenic veins, comes powerfully to aid the assimilation already commenced in the gastro-intestinal veins, as these last veins must be considered as branches at the same time absorbing and assimilating, while the others which have absorbed nothing are branches purely assimilating.

All these circumstances being thus borne in mind, it will be more easy to show the part which appears to me to belong to the spleen in the preliminary functions of hepatic sanguification. We shall see, indeed, as anatomy has already made to be anticipated, that the spleen comes, so to speak, to combine its functions with those of the portal vein.

If we reflect on the organs which send their venous blood into the current of the Portal Vein in order to augment the mass of that current, and thereby to insure the most complete assimilation of the matters absorbed, we see that in the most of these organs this function of assimilation is in some sense secondary; for it is added and annexed to other functions more important or more evident, as for instance, that of containing bile in the case of the gall-bladder, that of secreting the fat-dissolving, and emulsion-forming liquid in the case of the pancreas, that of fixing by a solid connection the digestive organs in a given position in the abdomen, in the instance of the epiploon and mesentery. We involuntarily then search for a special organ, in which the function of furnishing assimilating blood to the Portal Vein is an exclusive or principal object of organization. Now every thing concurs to show that such an organ exists, and that this organ is the spleen.

The reasons which tend to establish this functional object are the following.

First, The spleen is traversed by a very great quantity of blood. No organ, considered with regard to its volume, receives an artery so considerable. The splenic artery is in truth the largest branch from the coeliac trunk.

The spleen, though of glandular texture, secretes no particular liquid. Its duty in this way is confined to that of changing the arterial blood which it receives into venous blood, and into venous blood of a peculiar nature, as shall be presently seen. This venous blood is furnished in quantity much more considerable still than is the arterial blood which proceeds to the spleen. The Splenic Vein is four or five times larger than the artery of the same name, and it furnishes almost the half of the whole quantity of blood which traverses the trunk of the Portal Vein.

There is in the veins of the stomach a peculiar disposition which to me appears to indicate the assimilating function of the splenic venous blood. This is the arrangement of the veins of the large *Cul de Sac*, which hardly detached from the part of the stomach by which they are sent off, proceed by the shortest and most direct line under the name of *Vasa Breviora* towards the splenic vein with which they communicate. These *Vasa Breviora* of the great *Cul de Sac* of the stomach, it appears to me that we ought to understand, in the following manner. We ought to admit at first that these veins, by reason of the portion of the stomach from which they issue, absorb during the process of gastric-digestion a large quantity of the ingested substances. These substances which we suppose may be very active, being introduced into a current of blood, proportionally not very abundant, would have been badly assimilated by it, or even might have been capable of modifying or altering it, if they had been placed in the most speedy way possible in contact with a column of blood, virgin or new, for all reaction. It is probably with this intention that nature, instead of making the veins of the large *cul de sac* follow the whole length of the large arch of the stomach, before effecting their communication with the Portal Vein, has caused them to open by the shortest and most direct line into the splenic vein.

We have found in the nature of the Portal blood a proof of its assimilating property; we have in the like nature of the splenic blood the same proof of the same property. This resemblance in nature, the analyses of M. Beclard place beyond all doubt.

"Extracted by beating," says this observer, "the fibrine of the splenic blood is decolorized with much greater difficulty than the fibrine of arterial or venous blood; it is less elastic; it does not separate into ropy filaments, but into small masses which are agglutinated to each other. When left in contact with air, the splenic fibrine is at the end of twelve hours almost entirely liquified. The fibrine extracted at the same time from the blood of the jugular vein, and placed in the same atmospherical conditions, instead of being liquified, parts on the contrary with its water and is dried."

The blood of the splenic vein therefore is characterized with that of the portal vein, by containing a deliquescent fibrine; blood which consequently possesses little coagulating power, and is eminently well adapted to exert a solvent and assimilating action. This fact of chemical analysis completes the proof, as has been above said, in the estimation of MM. Bernard and Mialhe, that it is not necessary to consider as *albuminose* all soft fibrine found in the blood of the portal vein; for the reason that the splenic vein which does not issue from the digestive tube, and which accordingly cannot convey from that apparatus albuminose, furnishes, nevertheless, soft fibrine as well as the mesenteric vein.

This speedy transformation, in the interior of the spleen, of arterial blood into splenic venous blood, supposes a physiological operation analogous to that which takes place in glands. Now in point of fact this transformation is thus accomplished, since the spleen, as has been shown, is a glandular organ.

Physiologists, nevertheless, may not wish to allow that the simple transformation of red blood into modena blood, supposes a glandular action; and on this point they may maintain with Haller, the principle that there is no gland where a particular liquid is not secreted.

Upon this point we remark, that in the spleen there is not a simple transformation of arterial blood into ordinary venous blood. The splenic venous blood is a fluid of a peculiar nature. This is a fact long admitted by all physiologists and by Haller himself, though it was not precisely known that the character of the splenic venous blood is that it possesses a fibrine which is liquefied in the air, instead of being dried. It may therefore be asked what is to prevent us from thinking that this special blood is a species of ordinary venous blood, the fibrine of which is thus modified by a liquid secreted by the glandular grains of the spleen. And as it would be without purpose, that such a liquid should be secreted separately into particular ducts, nature mixes it with the venous blood as speedily as possible; that is to say, while it is in the state of forming particles, and thus causes it to flow in the same vessels.

Whatever may be thought of this hypothesis, which I offer merely as a means of enabling readers to comprehend the special nature of the splenic venous blood in its relations with the glandular structure, I think the question may be stated whether it is wise to say with Haller, "That no organ can be considered to be a gland which does not furnish a secreted liquid." These principles, laid down by scientific authority, to give force to certain facts which are thought to be well known, have, in my opinion, the inconvenience of desiring to render the future fixed, and accordingly to stop progress. Who knows, indeed, if, beside the glands properly s called, which secrete a particular liquid, there are not other sort of glands, as the spleen, the function of which is to produce species of venous blood, quite different from ordinary venou blood in its nature and uses.* We may mention, in passing on glandular organ, the liver, which unites in itself these two sorts of glands. The liver, indeed, which is a gland, properly so called, by the bile which it secretes, is further a gland analogous to the spleen, in so far as it secretes a sort of venous blood which contains sugar, fat, and other matters. It is also deserving attention, that, while the spleen produces a deliquescent and imperfectly coagulable fibrine which it sends towards the liver, the latter organ, by a species of inverted glandular elaboration, restores to this fibrine a degree of exquisite coagulability.

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* We ought to refer to this species of gland, the thyroid body, and the thym gland, the function of which would be to secrete a peculiar species of veno blood, and to pour it into the current of the *vena cava*, in order to modify t blood, and to prepare it probably for pulmonary *hæmatosis*.

We must not, however, forget, that the portal vein (mesenteric division) also gives, like the spleen, a form of venous blood, the fibrine of which is deliquescent. This blood is not less the result of a secreting or glandular process. To understand this, it must be remembered that the whole body of gastro-intestinal follicles may be considered as constituting a gland, the component grains of which are spread over a membrane. This gland secretes on the one side, the mucous covering of the alimentary canal, and on the other hand, by the rootlets of the gastro-intestinal veins, it sends in the same direction and with the same purpose as the spleen, a species of blood similar to that which proceeds from the splenic vein.

When I say that the blood of the portal vein and that of the mesenteric vein are alike, it must not be supposed that they are completely so. They are alike in so far as contains each a deliquescent fibrine, which is certainly a powerful reason of similitude; but they differ in another respect. Thus, according to M. Beclard, the splenic blood differs from the ordinary* venous blood from the jugular vein, for instance, in this respect, that it contains fewer globules and more albumen, while the blood of the mesenteric veins, on the other hand, contains more globules and less albumen. Though it be very difficult to assign the reason of this difference, we do not perceive that it can give rise to great consequences as to the organism. For as the splenic blood is intended to be mingled with that of the mesenteric vein, in order to form the column of blood which traverses the trunk of the Portal Vein, as the proportions of the albumen and of the globules, as has been just seen, are alternately inverse in the two species of blood; the mixture which is effected in the trunk of the portal vein ought to re-establish the equilibrium in the proportions of the albumen and of the globules, so that in this point of view, the composition of the two species of blood mingled can

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[It may here be observed, that, fully twenty years ago, Meckel, Professor of Anatomy at Halle, proposed the division of glands into Perfect and Imperfect Glands, understanding, by the latter, the spleen, the thyroid gland, the thymus gland, and the ovaries of the female.]

* Recherches experimentales Archives Generales, Octobre, Novembre, Decembre, 1848.

no longer be sensibly different from that of the ordinary venous blood.*

I have stated all the reasons which tend to establish the inference of the assimilating functions of the spleen, and which accordingly indicate this to be a very important associate to the gastro-intestinal veins.

We must, nevertheless, give a little attention to a theory of the functions of the spleen, which has not yet been under consideration. I refer to the theory of Tiedemann and Gmelin.

These physiologists are of opinion that the spleen serves to secrete a red coagulable lymph, which it transmits by its lymphatic vessels to the thoracic duct, in which it is to contribute to the assimilation and to the change of the ordinary characters of chyle into blood.† The mesenteric ganglions also act in the same manner, but in a degree less energetic. In other points, the reasoning upon which they found their theory is the following.

"We made the same remark upon the chyle which we collected from the lymphatic vessels of the intestinal tube in horses, before the entrance of these vessels into the mesenteric glands. This chyle was always white, and never reddish, and did not undergo coagulation; or, if it was coagulated, it was always slowly and imperfectly; whereas, the chyle, when it proceeded from the lymphatic vessels, which issue from the mesenteric ganglions, had a reddish colour, and it then became coagulated more easily and more completely. *Lastly*, it appeared much redder still in the thoracic duct, after the introduction of the red coagulable lymph which the lymphatic vessels from the spleen brought to this tube, and there, also, it underwent more prompt coagulation. From these observations, it accordingly results, that the chyle is purified by successive degrees by its passage in the mesenteric glands, and that by its mixture with the red coagulable lymph of the lymphatic vessels of the spleen, it approaches the blood in quality, and becomes more and more like this fluid."‡

From the considerations into which we have hitherto entered, we ought quite naturally to be led to admit this theory of Messrs Tiedemann and Gmelin, connecting it, however, with that which we have previously unfolded, in such manner, that, meanwhile, the spleen must appear to be an important organ, charged with a two-fold assimilation. On the one hand, by the venous blood which it furnishes, it modifies the alimentary substances which have been introduced into the Portal system, and disposes them to undergo the elaboration of hepatic sanguification. On the other hand, by the peculiar lymph which it transmits to the thoracic duct, it equally modifies the chyle, that is the fatty portion of the alimentary articles, and causes them to approach to the nature of blood before this change is completed in the respiratory function. In this double assimilation, however, venous and lymphatic, it is to be remarked that the spleen does not act alone; this organ receives powerful assistance on the side of the Portal system, from the blood of the other venous branches besides the splenic vein, and on the side of the chyliferous vessels from the lymphatic glands of the mesentery. This point shall again come under consideration.

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* M. Beclard proceeding on this inequality in the proportion of the globules presented in the splenic blood and the mesenteric blood, admits that the globules are destroyed in the spleen, and are formed in the mesenteric vein. It is in other points to him a simple opinion, surprising in the present state of science, and thus he thinks he can make it be accepted only when he shall have shown the laws which regulate the movements of the Portal Blood.

† Die Verdauung Nach Versuche. Heidelberg, 1826.

‡ Die Verdauung Nach Versuche. Heidelberg, 1826. Page 92.

I have not yet quoted a theory of the functions of the spleen, lately promulgated, and sufficiently allied to that which I here propose. M. Berard is of opinion, that the mass of blood which issues from the spleen goes to be added to that which arrives loaded with alimentary substances from the gastro-enteric veins, and this with the single purpose of diminishing the relative proportion of heterogeneous materials which this blood conveys in its stream.* There ought to be in the splenic blood more than this function of simple mingling; for if it is true that the blood possesses the property of modifying and destroying certain substances which have been ingested, everything leads us to infer that this property resides also in the blood, which is placed first in contact with the aliments, that is to say, the blood of the Portal vein and of the spleen. From this assimilating influence of the blood, however, we must except those substances which are not susceptible of assimilation; for instance, mannite (Mialhe), or energetic substances taken in large quantity, as alkohol in the case of intoxication.

Hitherto we have studied in the spleen only the function which it performs by the aid of its two fluids, venous and lymphy. But, as is known, the spleen is not only a gland; it is a gland dilatable and retractile. It is requisite then for us to show what uses are fulfilled by the vasculo-areolar element, to which, as well as to its investing membrane, it owes the remarkable faculty of being capable of being dilated and again contracting itself.

For this purpose, I cannot do better than cite an experiment well known, simple, and easily repeated.

After having exposed in a living animal the trunk of the Portal Vein, we compress it between the fingers, or a pair of forceps, in such a manner as to interrupt the continuous movement of the venous current, which is proceeding toward the liver. We observe then a swelling of the Portal Vein, and of its branches above the point compressed, which swelling is proportional to the degree of constriction employed; but we observe further a very apparent swelling of the spleen, which may be greatly augmented in volume. When we cease to compress the Portal Vein, the dilated branches are emptied at the same time as the spleen, which resumes all at once its original size, expelling by one single jet the blood by which it was distended.

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* Cours de Physiologie. Par M. Berard. Tome ii., p. 552.

It is easy to understand what takes place in this experiment. I shall, nevertheless, state its successive stages, the details of which are important.

We have first to notice the obstacle which the venous current encounters at the point of compression. The blood stopped and obstructed at this point increases in its mass, by the total quantity which necessarily is incessantly flowing by the original or convergent branches of the Portal Vein. Hence the dilatation of the veins placed above the point of compression; hence the dilatation of the spleen which is distensible like the veins, by reason of its vasculo-areolar network, and which further receives from the splenic artery a large quantity of blood, which the Portal Vein obstructed by compression can no longer receive.

From this dilatation results both to the veins and to the spleen, tension of the *parietes* with tendency to contraction. This contraction which is elastic in the veins, appears to me to be contractile in the spleen, particularly if we are guided by recent experiments.* By this property of contraction, the veins and especially the spleen, expel by a single jet the excess of blood by which they are distended, and immediately recover their original volume.

This impediment causes dilatation: dilatation induces tension, with tendency to contract; and tension becomes a force with which the veins, and especially the spleen, contend against the obstacle which opposes the current of the Portal Vein.

The obstacle now under consideration depends, as has been said, on compression of the trunk of the Portal Vein produced by the fingers or the forceps. There are, however, other obstacles quite as real, though not acting in the same manner, which induce the same results.

Thus, whenever respiration is suspended, as in making great efforts running, during the tetanic rigidity and similar states, and when the large venous trunks are not emptied by inspiration,† the inferior

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* The Society of Biology has recently proved, that we may produce a remarkable degree of contraction in the spleen, by subjecting it in a living animal to the contact of the electro-magnetic apparatus.—Memoires de Societe de Biologie. T. L. p. 157.

† See, on this subject, an interesting Memoir by M. Berard, entitled, Effects of the Elasticity of the Lung.—Archives Generales de Medecine. Juin 1830.

Vena Cava distended to excess is no longer able to admit the blood of the infra-hepatic veins. There is, accordingly, in this case, an obstacle or obstruction at the outlet of the infra-hepatic veins, and progressively at the hepatic capillaries of the Portal Vein, the trunk of this vein, in the branches from which it is formed, and to the spleen. But considering that the blood continues always to flow by the convergent branches of the Portal Vein and by the spleen, the result is a degree of distension of these vessels and of the spleen As a consequence of this distension, and in proportion to its intensity, there is developed a tension by reaction, elastic in the Portal Vein, contractile in the spleen, which struggles against the obstacle placed at the outlet of the sub-hepatic veins. It is thus that we must understand those dilatations of the spleen, mentioned by M. Segalas, as taking place during the accessions of intermittent tetanos, induced in dogs by the administration of *nux vomica.*[*]

But the species of obstruction the most important to be known is that which arises from the excessive plethora induced in the blood of the Portal Vein by the absorption of alimentary articles and drinks. This superabundance forms, during the time of digestion, a mass which interrupts the continuous movement of the current of blood, and resists, in a degree more or less considerable, the afflux of blood sent by the spleen and the original branches of the Portal Vein. As has been seen, when we were treating of other impediments and obstacles, these veins, and especially the spleen, undergo distension. They acquire a degree of tension by the aid of which they resist, forcibly and successfully, against the excessive fulness, which tends to stop the movement of the column of blood.

The obstacle, by excess, arising from the introduction of aliments and drinks, may be combined with that obstacle which depends on the defect of the flowing of the sub-hepatic veins into the *vena cava*, and of which we have above spoken. This combination takes place habitually in persons who run after having taken food. In this case, the fulness of the whole spleno-hepatic system is carried to the highest degree. The cutting pain of the side, which is then felt in the seat of the spleen, or towards the liver, and which forces the individual to stop, is nothing else but a splenic or hepatic pain, dependent on the intensity of the venous congestion. This congestion, though it may be very considerable in the liver, and may cause much pain, does not, however, distend this organ as it does the spleen, because the liver possesses, not like the spleen, an anatomical structure, which allows its volume easily to be increased.

Before quitting this subject, it may be proper to remark that the disposition of the vessels in the liver shows, that Nature has taken precautions to obviate the dangers arising from this excess of blood in the gland. Two considerations deserve notice.

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[*] Archives Generales de Medecine, T. xii. p. 138.

1*st*, We may observe the presence of the Capsule of Glisson around the trunk of the Portal Vein and its hepatic branches. Of this capsule, which thus doubles the wall of the Portal Vein towards one point, where it requires to support the whole effect of the reaction of its convergent and abdominal branches and that of the spleen, the evident object is to limit its movement of expansion, in the instances of superabundance, in order to obviate any hurtful compression of the portion of the hepatic parenchyma, which is contiguous to the trunk of the vein and its terminal branches.

2*d*, Means of derivation are provided for diminishing the excess of blood when it is threatening to be injurious. Such are the small vessels recently discovered by M. Bernard. These vessels establish a direct communication between the rootlets of the Portal Vein and the part of the *vena cava* which is adherent to the liver. Thence they allow to escape the excessive fulness of the Portal Vein, and pour this excess of blood into the *vena cava*, without causing it to pass through the sub-hepatic veins. M. Bernard has established as a fact the existence of these vessels, especially in the horse. The use of this arrangement is understood without difficulty, when we reflect on the domestic habits of this animal, which is frequently obliged to run immediately after a copious meal, and which, consequently, is thus placed in the condition most proper to induce a considerable degree of fulness in the Portal System.

We return to the subject of distensions of the spleen.

From the foregoing analytical exposition, we draw the following conclusions as to the occurrence, manner, causes, and purposes, of distensions of the spleen.

1*st*, When there is an obstruction to the progressive motion of the blood in the Portal System, the spleen is less distended by the entrance of the blood, which returns to it by a retrograde course, than by the afflux of that which is brought to it in abundance by the splenic artery.

2*d*, The spleen is then converted into a reservoir with tense, yet contractile walls, with a power of reaction proportioned to the quantity of blood accumulated, and able thus to contend against the obstacle which retards the normal progressive movement of the blood.

3*d*, This contractile impelling agent was particularly necessary in those cases in which a proportion, often considerable, of alimentary substances being added to the blood of the *vena portæ*, produces a mass of fluid difficult to be moved.

4th, Consequently the spleen, by reason of its vasculo-areolar structure, and by virtue of investing membrane, fulfils, as the Portal System, the office of a true heart with continuous impulse.

5th, This heart, multilocular, is, as it were, improvised, or immediately formed, on all occasions, when the current of the Portal Blood, being stopped in its movement, has need of an increase of propelling force, in order to enable it to traverse the liver.

6th, Excepting on the occasions now specified, this heart has no existence, because the simple *vis a tergo* is sufficient to cause the progressive motion of the current of blood. It is, in short, called, as already stated, into existence only on particular occasions of obstructed circulation.

A very simple experiment is sufficient to show the difference of impulsive action given by the spleen, according as the organ is distended or not distended. If, during its state of contractile dilatation, we make a puncture in the splenic veins, we see a jet of blood forcibly projected from the wound. When, on the contrary, the spleen is in its condition of ordinary size without dilatation, and a puncture is made in the splenic vein, the blood issues from the aperture only by trickling or in an insignificant jet.

The different physiological authorities have not taken the view now given of the purpose of the vasculo-areolar reservoir of the spleen. They have seen in this reservoir only the blood which is accumulated in it during digestion or during great muscular efforts; and they have represented the function of the spleen accordingly to be, to form a temporary vessel of reserve for any excessive fulness of blood; that is to say, that the spleen is a *Diverticulum* for the blood.

Without denying completely the theory thus generally received of the spleen forming a *Diverticulum Sanguinis*, I observe, however, that the accumulation of blood in the spleen is never the effect of a spontaneous or active action of this organ. On all the occasions on which the blood is accumulated in the spleen, in the normal state, this accumulation depends on an obstacle situate at the outlet of the sub-hepatic veins, in the liver, or in the Portal Vein; and in this case the increase in volume affects not only the spleen, but further, all the affluent branches of the Portal Vein. We readily admit that, under the mechanical influence of preceding circumstances, less inconvenience results to the organism from the blood being accumulated in the spleen than elsewhere; but this is not enough. We ought further to recognize the fact, that the accumulation of blood in the contractile reservoir of the spleen ought to contribute powerfully, as has been said, to accelerate the movement of the Portal blood, which is then more or less obstructed in its progress.

The spleen is not indispensable to life. This is sufficiently proved by the instances of persons who have lived without spleen, and by those experiments in which this organ has been extirpated from certain animals, for instance dogs, cats, guinea pigs, &c., which, notwithstanding this deprivation, have nevertheless continued to live, without presenting any remarkable derangement in their health.

This circumstance possesses nothing which ought to astonish the physiologist; because in the analytic estimate which we have made of its functions, we have seen that the spleen has auxiliary organs, which, when necessity demands, can perform its functions. Thus we have said the spleen produces a species of venous blood, with deliquescent fibrine, which assimilates and incorporates with the blood the ingested alimentary articles during their passage into the Portal Vein. Certainly this is the case. But the blood of the Portal Vein itself, which is remarkable for containing a similar deliquescent fibrine, produce the same effects of assimilation and incorporation. The spleen, say Messrs Tiedemann and Gmelin, secretes a red coagulable lymph, which, introduced into the thoracic duct, serves to assimilate the chyle and convert it into blood. True; but the same physiologists add, the mesenteric glands act also in the same manner, though in a degree less energetic.

Lastly we say, the spleen, in the instances in which an obstacle to the course of the Portal blood is formed, is converted into a contractile reservoir, which reacting on the blood accumulated, with an intensity proportioned to the magnitude of the obstacle, impels it in the direction of the liver, thus re-establishing the degree of velocity necessary to the current of the Portal blood. This is also true; but all the converging or abdominal branches of the Portal Vein, besides the splenic vein, which in the same circumstances may be dilated to the extent of doubling their usual volume, and the sum of which constitutes a reservoir sufficiently spacious, act by virtue of their elasticity, as the spleen does by its force of retractile tonicity, in order to produce the same results; and these vessels may then to a certain degree supplement the action of the spleen in the exercise of this function.

Thus then the spleen, which is an organ in two senses assimilating, in the Portal Vein and in the lymphatics; which further is, an agent accelerating the Porto-hepatic circulation, the spleen, I repeat, has supplementary assistants in this threefold function, of which it is the eminent organ, the organ in some degree titular.

This physiological condition is found not only in the spleen.

We know that certain organs, though they may be specially charged with the performance of one function, may be wanting, like the spleen, without thereby causing the suspension of the function. Thus the crystalline lens and the tympanal membrane serve the one for vision, the other for hearing; but, nevertheless, the removal of the crystalline lens, and the perforation of the tympanal membrane, do not, in consequence, deprive the eye of the faculty of seeing, and the ear of the power of hearing; vision and hearing are merely less perfect than they previously were. Consequently, the crystalline lens in vision, and the tympanal membrane in hearing, have not usages essentially necessary; they perform functions of precision and intensiveness. In other respects, this possesses nothing astonishing. In machines constructed by human art, we see certain parts essential, and certain mechanical details made to ensure precision, and to communicate energy of action and intensiveness. The same may be expected to take place in the arrangements of organised bodies, the most perfect machines that can be imagined.

From these considerations, we are led to the inference, that, if the spleen is not essential to the continuance of life, it must certainly be so to the maintenance of good and perfect health. We do not, however, find in this subject all the details desirable in the relation of the experiments which have been performed upon the bodies of animals, Experimentalists have been content with making the observation, that a particular dog, from which the spleen had been extracted by excision, and which survived the first effects of the operation, continued thereafter wonderfully well, without presenting anything unusual in his health. But did they subject this mutilated animal to all sorts of trials, to forced exercise, to fasting, to copious meals, to a particular mode of feeding, &c.? This is what inquirers have not taken the trouble to investigate.

I know scarcely any one but Dr Dobson, whose observations present any interest under the view now mentioned. This experimentalist having seen, in dogs, that the maximum of swelling of the spleen took place five hours after the ingestion of alimentary articles, took the idea to observe what was going on at this hour of digestion in dogs deprived of the spleen. He remarked at first, that, so long as the animal eat little at a time, nothing extraordinary took place; the animal continued well on such regimen, and even became fat. But very different was the result when the food had been given in large quantity. Five hours after feeding, signs of uneasiness were observed to appear; torpor, groaning, difficult breathing, frequency of calls to the urinary secretion, &c., which lasted in this manner for two hours before they were completely dispersed. That this assemblage of symptoms was not an imaginary result of preconceived ideas in Dr Dobson, is proved by the fact, that the dogs which he fed in this manner, by means of large meals given at remote intervals, pined away gradually, and died at the end of one month.*

All these symptoms of fulness, the pining away and the death of the animals, Dr Dobson explains, by referring them to his theory, which is, that the spleen is a *diverticulum* or reservoir intended to receive the excess of blood, which flows to the stomach during the entrance of food into the blood-vessels. I would say, that the symptoms of plethora and fulness depend on this fact, that there is in the Portal System at this time a mass of blood loaded with an excess of alimentary substances, which, failing the continued impulse of the spleen, encountered great difficulty in getting through the capillaries of the liver, and consequently distended the Portal Vein.† As to the mortal pining, which terminated these copious meals at the end of one month, the explanation of it is to be given by reflecting on the great excess of alimentary substances which no longer found either in the Portal Vein or in the lymphatics a sufficient amount of assimilating liquid. The assimilation was accordingly imperfect; the elaboration by the liver was badly performed; and, consequently, nutrition was defective. Hence the wasting and death.

As may be well imagined, it would be particularly important to observe in the human race the phenomena which show their presence after the extraction of the spleen; for we might have, in certain abnormal sensations, information which is completely wanting, when we reason from experiments performed on animals. The instances, however, of removal of the spleen in the human race are extremely rare. I know only two authentic examples, that is to say, cases in which the fact of the absence of the spleen was ascertained by inspection after death.

The first case is related by Fantoni. This was observed in the person of a female, who, after extirpation of the spleen, enjoyed a degree of health sufficiently good to become pregnant.

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* London Medical and Surgical Journal, 1830, and Archives Generales de Medecine, Nov. 1830.

† Among the symptoms of fulness enumerated above, and observed by Mr Dobson, the frequent excretion of urine deserves particular attention. This phenomenon has a peculiar value in reference to the ideas of M. Bernard, who admits that, in the instances of fulness of the Portal System, the excess which may have entered the *vena cava* regurgitates by that vessel as far as the kidneys, in which it is separated from the blood in the shape of urine.

The symptoms of fulness before-mentioned would have been much more considerable, if the dog, deprived of the spleen, had been obliged to run after taking his copious meals. The spleen does not restrain running, as has been said; on the contrary, it favours that act, by accelerating the movement of the blood in the Portal Vein.

and be delivered at the full time. Subsequent to this period, nevertheless, the abdomen was the seat of an unwonted swelling; she had attacks of Erysipelas every year, either on the head or some part of the person, and she was often affected by attacks of uterine hemorrhage. She died of an attack of Erysipelas five years after the extirpation which she had undergone. On inspecting the body after death, along with absence of the spleen, it was found that the liver was of extraordinary volume.*

The second example is more modern. It was observed, in 1844, by M. Berthet, physician at Gray (Upper Soane), and by him communicated to the Academy of Medicine. The case was that of a man in the prime of life, who received, in a fight, a wound by a knife, which laid open the right side. The spleen was protruded, was exposed to the air for some days, and became putrified. M. Berthet inclosed in a ligature the protruded portion and cut it off. The wounded man got well, and lived thirteen years and a-half afterwards, enjoying good health. The digestion was performed in general well. This man died of an attack of pneumonia.

Such are the principal details of these two cases. It must be allowed that they are very incomplete. They, nevertheless, permit us to maintain the point, that, in the human race, the removal of the spleen by excision induces disturbance in the organism. In the second person, the digestions are remarked to have been performed in general well; consequently they were sometimes badly performed. It would have been curious to know in what circumstances of alimentary dieting, and at what time of digestion these disturbances took place.

[Part Second, comprehending the Pathology of the Spleno-Hepatic System in Next Number].

[In giving insertion to the Memoir of M. Beau, it is almost unnecessary to say, that we express no opinion on the merits of that Memoir, or on the accuracy of the facts and the validity of the arguments by which he sustains the different propositions of his hypothesis. For these the author alone is responsible. All that belongs to us is to make known these opinions and doctrines, in order that they may be examined dispassionately and deliberately.

On several points, the views of M. Beau possess the merit of directing attention to the great influence of the gastro-enteric veins in absorbing the products of alimentary digestion and various fluids introduced into the alimentary canal. These views, though not new, may be serviceable by directing attention to the great influence both of different kinds of food in maintaining and deranging health, and, above all, to the action of stimulating drinks, both directly upon the gastro-enteric tissues, and indirectly upon the tissues of the veins, the blood flowing within these veins and the structure of the organs, especially the liver, through which these substances are conveyed and distributed by the blood. The absorption of various articles ingested, especially in the liquid form, by these veins, is a fact which has been long familiar to the minds of physiologists. But the systematic and extended application of this doctrine, in the manner in which it has been done by M. Beau, may be useful in directing the attention of physicians to the powerful influence of these veins, both for good and evil, in the animal economy. The latter part of the doctrine is more fully elucidated in the second part of the Memoir.

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* Fantoni Opuscula Medica, p. 195–203. 1738.

One part of the doctrine of M. Beau may not be so readily received as that to which reference has been already made. It is that which relates to the assimilating power which he ascribes to the blood of the Portal Vein. In the usual doctrine the assimilating power, such as it was, was ascribed to the venous blood of the superior cava. But this was supposed not to be considerable, or rather to be non-existent, until the blood was subjected to the aerating action of the lungs, to which organs accordingly the assimilating function was principally ascribed. According to the doctrine of M. Beau this process begins a step earlier; and, though it may receive its final completion in the blood-vessels of the lungs, it is commenced, according to him, and in a great degree performed in the vessels of the Liver. A question naturally arises;—What are the qualities of blood which enable this fluid to exert assimilating powers? |

ART. V.—*On Some Points connected with the British Naval-Medical Department and that of the United States.* By THOMAS STRATTON, M.D. Edin.; Surgeon Royal Navy.

IT is interesting to compare the arrangements in one service with those in another, and with this view I beg to offer to the readers of this extensively-circulated Journal a short notice of the medical department of the United States' Navy.

On the first of January, 1851, there were in the United States' Navy 150 medical officers, of whom 69 were surgeons (including fleet-surgeons), and 81 assistant-surgeons, of whom 38 were passed-assistant-surgeons.

The pay of these officers in their several ranks and durations of service is exhibited in the following table.

United States Navy.		Dollars a-year.	Equal to Sterling.			Remarks.
			£	s.	D.	
Assistant-Surgeons, 43 in number,	Waiting orders, ...	650	135	8	0	A.
	At navy-yards, ...	950	197	18	4	
	In sea-service,......	950	197	18	4	
Passed Assistant-Surgeons, 38 in number,	Waiting orders, ...	850	177	1	8	B.
	At navy-yards,......	1150	239	11	8	
	In sea-service, ...	1200	260	8	4	
Surgeons. First five years,	In commission, ...	1000	208	6	8	C.
	At navy-yards, ...	1250	260	8	4	
	In sea-service,......	1333	277	17	2	
	Of the fleet,.........	1500	312	12	0	
Surgeons. Second five years,	On leave,........ ...	1200	250	0	0	
	At navy-yards, ...	1500	312	10	0	
	In sea-service, ...	1600	333	6	8	
	Of the fleet,.........	1800	375	0	0	
Surgeons. Third five years,	On leave,....	1400	291	13	4	
	At navy-yards,......	1600	333	6	8	
	In sea-service,......	1750	364	11	8	
	Of the fleet,.........	2100	437	10	0	
Surgeons. Fourth five years,	On leave,............	1600	333	6	8	
	At navy-yards, ...	2000	416	13	4	
	In sea-service,......	2133	444	10	8	
	Of the fleet,.........	2400	500	0	0	
Surgeons. Twenty years and upwards,	On leave,............	1800	375	0	0	
	At navy-yards, ...	2250	468	15	0	
	In sea-service,......	2400	500	0	0	
	Of the fleet,.........	2700	562	10	0	

In addition to this, there is on shore and at sea a ration a-day, of value of ten cents or fivepence. These particulars are taken from the *American Almanac* for 1851, published by Little and Brown, Boston (page 122). The dollar is here reckoned at 4s. 2d. sterling.

The *waiting orders* and the *on leave* in the American navy correspond to the *being on half-pay* in the British navy.

Remark A. In the British navy an assistant-surgeon's half-pay for the first three years is 2s. a-day, or £36, 10s. a-year. This pay is about £100 a-year less than that of the American unpassed-assistants. The full pay is about £70 a-year less than the American. In the Royal Navy the full pay of assistant-surgeons is, under three years, 7s. a-day, or £127, 15s. a-year; above three years, 7s. 6d., or £136, 17s. a-year; above ten years, 9s., or £164, 5s. a-year; in ships which do not carry a full-surgeon, or *in charge*, under ten years, 8s. a-day, or £146 a-year; and above ten years, 10s. a-day, or £182, 10s. a-year.

Remark B. In the royal navy the length of time an assistant-surgeon serves before being promoted is usually between nine and ten years; he must serve three years before he is eligible for promotion, and he is then examined. About two-thirds of the assistants of the British navy correspond to the passed-assistants of the United States' navy.

In the royal navy the halfpay of assistants after three years, is 3s. a-day, or £54, 15s. a-year; this is about £140 a-year less than what it is in the United States' navy.

In the royal navy assistants who are both *in charge* and above ten years' service receive £182, 10s.; but there are few of this seniority so situated, and this amount of pay may be left out of consideration. What all, or nearly all, receive is between £136 and £146; this pay is to be contrasted with £250 in the United States' Navy.

In the United States' Navy *all* the assistants have cabins and mess with the lieutenants.

Remark C. The authority I have quoted uses the expression *in commission*, but it would appear from the context to be used here in the same sense as *waiting orders* and *on leave.*

In the British navy a surgeon's half-pay is, under six years, 5s. a-day, or £91, 5s. a-year; above six years, 6s. a-day, or £109, 10s. a-year; above ten years, 7s. a day, or £127, 15s. a-year; above fifteen years, 8s. a-day, or £146 a-year; above twenty years, 10s. a-day, or £182, 10s. a-year; above twenty-five years, with leave to retire, 13s. a-day, or £237, 5s. a-year; after thirty years' service, with leave to retire, 15s. a-day, or £273, 15s. a-year.

It may be mentioned here, that very few indeed have been able to serve thirty years; few have served twenty-five years; even those who have succeeded in serving twenty years are seldom to be met with; so that, perhaps, nineteen in every twenty are not receiving more than £146 a-year of half-pay. In the British naval medical department, the period of service is so broken up by intervals of half-pay consequent on the triennial *paying off* of ships, that it generally takes a surgeon forty-five years, or perhaps fifty-five years to serve twenty-five years, (as it only full-pay service which reckons).

The various rates of surgeons' half-pay, annually, are in the

British Navy.	United States Navy.
£ 91	£208
109	250
127	290
146	333
182	375
237	375
273	375

In the above view sums under a pound are omitted in the yearly pay.

We now come to the full-pay of surgeons, which in the British navy is,—under six years, 11s. a-day, or £200, 15s. a-year; above six years, 12s. a-day, or £212 a-year; above ten years, 14s. a-day, or £255, 10s. a-year; above twenty years' service, 18s. a-day, or £328, 10s. a-year.

By comparing this with the first table, it will be seen that the pay in the United States' navy is much larger than in the British navy; it is in some instances about one-half more, and in others about double.

As to the comparative expense of living in the two services, this is either abroad or at home; abroad, it may be supposed to be the same; at home, as living is less expensive in America than in Britain, the larger American pay is employed to meet the lesser expense.

It would be an advantage to have in the royal navy, a class of officers to be denominated *Fleet Surgeons;* they might correspond in rank to the first-class-staff-surgeons in the army, ranking with majors in the army, and commanders in the navy, according to the dates of their respective commissions; a surgeon to be eligible for promotion to this rank after three years' service. It need not necessarily follow from his name, that he should do any more duty than that of surgeon of the ship to which he might be appointed, or be inelegible for any situations on shore.

In Dr Barton's Treatise on Marine Hospitals,* are many interesting observations on the details of a medical department for a public service; his work was published in 1814, at which time there were 41 surgeons and 44 assistant-surgeons in the United States Navy. Dr Barton approves of placing medical institutions, such as hospitals, under the supervision of non-medical officials. Although I had the pleasure of meeting Dr Barton at Philadelphia in 1841, I do not know if he has changed his opinion on this subject, but if he have not, I beg most decidedly to differ from him.

It may be somewhat interesting to army medical readers to quote from the same authority (*American Almanac*, page 120), the rates of pay of the medical officers in the United States army. Besides the daily pay, there is reckoned separately an allowance for rations, for servants, and for forage. *All* assistant-surgeons are allowed forage. The Surgeon-General has 2500 dollars a-year, not including allowances; his total monthly pay, including allowances, is 208 dollars, 33 cents, or £168, 6s. sterling, for a month of thirty days, or £5, 12s. 2d. a-day, being £2047, 12s. a-year.

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* A Treatise containing a Plan for the Internal Organization and Government of Marine Hospitals in the United States; together with a Scheme for Amending and Systematizing the Medical Department of the Navy. By Wm. P. C. Barton, M.D., Surgeon United States, Navy. Philadelphia, 1814. 8vo. Pp. 244.

[It may be observed here, that the medical Director-General of the British Army lately retired on his full-pay of £2000 a-year; less than the above, although from the one army being so much larger than the other, the amount of duty must have been much greater.]

The *total monthly pay*, that is, including allowances, of surgeons of ten years' service, is 149 dollars, or about 20s. 8d. sterling a-day.

Surgeons under ten years, receive 125 dollars, or about 17s. 4d. sterling a-day.

Assistant-surgeons, of ten years' service, receive 122 dollars, 50 cents a-month, or about 17s. 2d. sterling a-day.

Assistant-surgeons of five years' service, receive 98 dollars, 50 cents a-month, or about 13s. 9d. sterling a-day.

Assistant-surgeons, under five years' service, receive 81 dollars and 83 cents per month, of thirty days, which is equal to about 11s. 3d. sterling a-day.

I am afraid that some readers will consider that these details are somewhat misplaced in a Journal so scientific as the *Edinburgh Medical and Surgical;* if such there be, to them I offer as an apology the reflection that the successful cultivation of science depends a good deal on the degree in which it is rewarded in a pecuniary as well as in an honorary[*] manner.

I trust that in a communication where so many figures occur, and which will not be corrected by the writer, the printer will be careful not to make any errors for the candid reader to pardon.

PRINCE-EDWARD ISLAND,
October, 1851.

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[*] See on this subject the eloquently written pamphlet entitled *Summary of the Claims of Medical Officers of the Army and Navy to Military Rewards and Distinctions.* London; Churchill. 1849. Pp. 24. Reprinted from Lancet of 22d September.

Erratum in Dr Stratton's Paper in Vol. lxxv.
Page 292, line 24, *for* two ounces *read* two drachms.

ART. VI.—*Remarks on Death from Strangulation.* By ALFRED S. TAYLOR, M.D., F.R.S., Lecturer on Chemistry and Medical Jurisprudence in Guy's Hospital. [Guy's Hospital Reports, Vol. vii. Part ii. p. 371.]

Medical evidence in a case of murder by strangulation.—On the cause of death; determination of the period of death by the cooling of the body; rules for determining whether the act of strangulation was homicidal or suicidal.—Medical and circumstantial proofs of homicide; comparison of cases of suicidal strangulation.—Period of time within which death from strangulation takes place; time for the loss of consciousness and muscular power; false analogies.—Chemical and Microscopical examination of blood-stains on clothing.—Special characters of Mammalian blood; resemblance of the blood of man and Mammalia.—Recent and old blood-stains; evidence of attempted removal; influence of dyes in articles of dress on blood and red colouring matters.—Chemical differences between blood and mineral, vegetable, and animal colours.

AMONG the Medico-legal cases which have of late years been tried in this country, few have excited greater interest than that of THOMAS DRORY for the murder, by strangulation, of a female named JAEL DENNY. The trial took place at the Chelmsford Lent Assizes for 1851, before Lord Chief Justice Campbell; and involved medical considerations of the highest importance, in reference to physiology, chemistry, and microscopical research. The prisoner had the advantage of being defended by counsel of great ability.* The crime which he had committed was witnessed by no one; and he had so laid his plans as, in his own belief, to defeat the efforts of the law to bring it home to him. The proof of the murder rested only upon circumstances, and on medical inferences based on well-ascertained facts; but in spite of the great legal ingenuity exercised in the defence, aided, as it was, by medical opinions adverse to those taken by the witnesses for the prosecution, the prisoner was convicted; and the correctness of the verdict was fully vindicated, by the fact that he made a confession of his guilt before execution.

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* Counsel for the prosecution, Mr Edwin James, Q.C., and Mr Roduell:—for the defence, Mr Clarkson, Mr Bodkin, and Mr Hawkins.

The prisoner, Thomas Drory, was the son of a farmer of great respectability, and resided within a short distance of the cottage where the deceased Jael Denny lived. They were both of about the same age, namely, twenty years. The deceased, at the time of this occurrence, was pregnant by the prisoner, and had reached the ninth month of her pregnancy. On the afternoon of Saturday, October 12th, 1850, the prisoner and deceased were seen conversing together, for about twenty minutes, in the neighbourhood of the prisoner's cottage. This was about half-past five o'clock; they then parted, and went to their respective homes. The evidence respecting *the deceased*, showed that at about six o'clock on this day, she had tea with her parents as usual, appearing to be in good health and in high spirits. She told her mother that she had made an appointment with the prisoner, to meet him at a stile very near their cottage *at half-past six* o'clock; and the prisoner, it was supposed, had led her to expect, that at this interview he would make some arrangement regarding his marriage with her. At or about this time, the deceased left her tea half-finished, dressed herself hastily in some of her mother's clothing, left the house, and was not again seen alive.

Her parents sat up for her until a late hour, but she did not return. She was found lying dead in a field at or about *eight o'clock* on Sunday morning, October 13th, at a short distance from the stile at which she said she had made an appointment to meet the prisoner on the previous evening.

When her body was found, the head was cold, and the arms and legs cold and stiff; but the body (the abdomen) was perceptibly warm to the hand. It will be remarked, that from the time deceased was last seen alive, thirteen and a half hours had elapsed.

The *attitude* of the body, when found, is thus described by the different witnesses. The deceased was lying on her face, a little inclined on one side, owing, probably, to the prominence of the abdomen. Her lower clothes were arranged in a straight and orderly manner, and her fur-tippet was lying on the ground, two or three yards from the body. Her bonnet was on her head, but much crushed and broken. It was flattened in front, as if from pressure from behind while the deceased was on her face. Her face was flat on the ground, and her nose pressed down tightly. The nose is described as being quite flattened, and turned a little to the left side by pressure; it was impossible, in the opinion of one witness, that the mere weight of the head could have produced either this degree of pressure or the indentation observed in the ground. The features were so altered, that, although this witness had known the deceased for four or five years, he could not recognise her. When the body was turned over, blood escaped or bubbled from the mouth, nose, and eyes; and the face was observed to be black, and much swollen. There was half a teacupful of blood on the spot where the face lay—under the mouth; and more blood in another spot, about a foot from the head; the hair was matted together with blood and dirt. The right arm was lying bent at a right angle underneath the body, and pressed down by its weight; the left was raised, with the hand directed towards the left shoulder, but partly covered by the body. There was a cord on the neck, which was twisted round it three times. One of the witnesses took the third turn from off the neck, and observed that this turn was a little loose; but, on putting his finger to the throat, he found a knot of cord lying in front of the neck. The remainder of the cord was very tight, a portion being actually imbedded in the neck, and the cord was drawn so tightly, that the skin of the neck had swollen up between the coils. From other evidence it appeared, that the knot which formed the loop of the rope, was pressing on the fore part of the neck, while the bight of the noose was at the back part, a little behind the left ear. There were *three coils and a half* of rope round the neck, and, with the exception of the last half-coil, all were tight; the two innermost coils being so tight as to indent and cut the skin. The end of the cord went over the back of the left shoulder, and about an inch of its extremity was lying loosely (without being grasped) between the thumb and the finger of the *left* hand of the deceased, which was raised towards it. One witness described this hand as being stretched out a little, so that the end of the cord could be seen lying in the hand, before the body was moved or turned over. The deceased was *right-handed;* there was no mark of grasping, laceration, or indentation on either hand; and from the position of the bite of the noose and the direction of the coils, the cord could have been tightened only by pulling to the *left* of the deceased. The cord was stout, and of the thickness of a window sash-line. At the part where the noose had been tightened, the pressure had been so great that the cord was condensed to about half its thickness, and some of the fibres had been cut through by the force used. There was no blood upon it, except just at the end, where there was a small spot. The second coil had at the back part, tightly locked in a portion of the apron of the bonnet and handkerchief of the deceased.

A woman, who undressed the deceased six hours after the body was found, stated that she examined her face, and found the mouth bubbling with blood; her tongue protruded out of her mouth, and was clenched very tightly with her teeth. Blood oozed from her eyes, mouth, and ears. Her body, from her head to the shoulders, was very black (livid). There were two marks where the cord went round the neck, quite lacerated through the skin. Upon the back of her left wrist were marks, apparently of a bite, from both rows of teeth,—the impressions were quite distinct before they were washed, and blood was oozing from them. On the right elbow a piece of skin had been taken off, about the size of a shilling, and the patch was very black. The elbow had a bruised appearance.

A *post-mortem* examination of the body of the deceased was made by Mr Williams, surgeon, of Brentwood, on the second day after it was found. The eyes were much distended and suffused with blood, and the pupils were dilated. There was a general lividity and swelling of the face, and the tongue, which protruded from the mouth, had been bitten by the teeth. There was a superficial laceration of the skin covering the lower part of the throat on both sides, and there were two deep marks, as if from two cords, or from two impressions of one cord, tied tightly round the neck. The two impressions were both situated over the trachea, and the skin had swollen up between them. The trachea had been flattened by strong pressure, but had regained its shape; it had a bruised appearance in the parts corresponding to the two marks on the neck, and its structure there was softer than natural. There was extreme ecchymosis on the upper part of the chest, such as might have been produced by a heavy blow, or by the pressure of a person kneeling upon it. There was a contraction of the fingers, which were drawn in to the palms of the hands. There was an abrasion of skin at the back of the right elbow. There were marks, apparently of teeth, on the back of the *right* wrist, and there were also scratches on the back of the left arm and hand. On opening the head, there was great congestion of the whole of the brain. The heart was healthy, but much distended on the right side with blood in a coagulated state. The lungs were congested to an unnatural degree; the right pleura was adherent,—a result of previous inflammation. The stomach contained ordinary food, and the coats were in a healthy condition. The intestines were healthy. On opening the uterus, it was found to contain a male fœtus in the ninth month, and this was probably alive at the time of the deceased's death.

After having deposed to these facts at the trial, Mr Williams further stated, in answer to questions,—

"I believe the cause of death was strangulation; and from the marks of the coils of the rope on her neck, and the violence used, the deceased could not have done it with her own hands. That is my decided opinion.

"*By the Court.*—I draw the inference as to the violence from the marks on the body. I am speaking as a man of science; and from my knowledge of physiology, I say she could not do it herself.

"*By* Mr James.—When a rope is put tightly round the neck, the effect is almost immediate insensibility; compression of the trachea would produce immediate insensibility, and that undoubtedly would prevent the deceased passing the rope a second time round her neck; a person could not scream after the coil had been once round the neck, or make any loud noise to be heard; I have heard the evidence of the witnesses as to the state in which the body was found, and assuming that to be true, there must have been great violence used; and I believe no person could use such violence with her own hand.

"*Cross-examined by* Mr Clarkson.—I never saw a person who died by self-strangulation; I have never had an opportunity of comparing the appearance of a person who died by self-strangulation and violence; I think you could not have these marks of a rope round the neck of a self-strangled person. I should think the ecchymosis about the neck would be some guide to decide in such a case as to suicide by strangulation, or death by the violence of another. I did not notice any marks of teeth on the *left* hand of the deceased; I afterwards examined the prisoner's mouth, but I did not see anything about it to indicate that that mouth made the mark on the *right* hand of the deceased. I formed an opinion.

"*Lord* Campbell.—You say you formed an opinion; tell me on what?

" *Witness.*—From the configuration of his teeth.

"*Lord* Campbell.—From what you saw on the body you tested the configuration of the mouth.

" *Witness.*—I did, and the arrangement of the teeth very much corresponds with the mark on the hand.

"*Lord* Campbell.—Then why did you say you found nothing?

" *Witness.*—There was no blood on his mouth.

" *Lord* Campbell.—That is the reason why you said you saw nothing to connect them.

" *Witness.*—Yes.

"*Lord* Campbell.—What, from the conformation of the teeth, do you think you discover to form an opinion on?

" *Witness.*—I form the opinion from the regularity of the teeth, and the absence of indentation in the wound.

"*Professor* Taylor, *examined by* Mr James.—I have attended to the evidence given by the various witnesses as to the appearance of the deceased's body. I have particularly attended to the mode in which this cord was found round the neck of the deceased, to its being in her left hand, and to the fact of her right hand being under her; and from the appearances I am decidedly of opinion that it was not a case of suicide by her own hand. It could not have been done by herself while lying down, for she could not, in that position, have exerted force sufficient to form such marks on the neck. Taking the upright position, if the first coil was so tight round the neck as described by the witnesses, it would have speedily produced insensibility and loss of muscular power, so that a second coil could not have been tightened round the neck by her own hand, to have produced a second compression and a second laceration on the sides of the neck. Acting with the *left* hand, there would be a very speedy loss of the power of tightening a cord with a noose already formed, unless the cord had a single end with which she could draw it, and it were firmly turned round the hand several times to give a firm hold. From the immediate insensibility which would have followed the complete compression of the windpipe, she would have been found dead, with the end of the cord twisted round the left hand, or some mark on the left hand, of the cord having been so round. I do not believe that the marks on the windpipe could have been caused in any other way by herself than by the end of the cord being thus coiled around her hand or attached to some fixed point; and in this case the last coil ought to have been just as tightly drawn as the two first coils. In self-strangulation, so soon as the windpipe is firmly compressed by whatever cause, the individual becomes unconscious, and the person can neither tighten the cord nor loosen it. We thus have the power of judging from the position of the body and the state of the cord, how far it could have been an act of suicide. I give it as my firm opinion that this could not have been an act of suicide.

" If she were lying down or standing up and the cord was round her throat, she could not cry out or make any noise. Directly the windpipe is compressed an individual becomes powerless, and incapable of making any cry. Assuming a person lying down with a cord tightly bound round her neck, in half a minute she would become quite powerless.

[" I had submitted to me the breeches of the prisoner with blood upon them; I removed the patches with blood upon them for examination; there were several dark-coloured stains on the breeches; I noticed when I received them that the stains were dry. The largest stain was upon the front of the thigh. One patch had the appearance of having been rubbed: it had been caused by a red liquid, which had soaked through the stuff. There was a stain near the left pocket-hole; it had the appearance, superficially, of a smear of blood. There were other small spots at the bend of the knee behind. There were nine or ten well-marked stains upon the clothes. One mark was a fruit-stain, but all the other marks were blood. There is no power of deciding whether they are recent spots or not; they might have been on the corduroy twelve or fifteen hours, or for several weeks or months. I think the large spot has only been rubbed; it might present the same appearance if anything damp had been applied to it. I had a jar with some substance in it, given to me for examination. The substance is called catechu, and is employed as an astringent for animals and human beings. I saw no stains on the breeches produced by the action of catechu.

" *By his Lordship.*—I have applied both microscopical and chemical tests, and they are spots of blood.

" *Cross-examined by Mr* Bodkin.—I find the blood has been removed from the furrows of the corduroy, but not from the ridges, which might have been caused by the wear or rubbing; I have put some catechu upon a similar piece, it turns olive-brown, and not like blood. There is no clear distinction between the blood of man and quadrupeds, but the blood of fish and fowls may be distinguished.]

" *Mr* Bodkin.—Assuming this young girl to have been standing up, and that she died by self-strangulation, would she not fall forward? *Witness.*—If she was pregnant there would be a disposition to fall forward.

" And in consequence of its being sudden, the fall would be likely ~~to be~~ heavy?—It would.

" Would it be likely for a person falling under such circumstances to injure her nose?—Possibly, on a hard surface; on a soft surface I should say no.

" The nose would strike against the ground, and the state of injury would depend on the nature of the soil?—It would; but it would cause ecchymosis, not flattening—I should say that in falling there would probably be the mark of a bruise on the face, according to the force of the fall.

" You assume the description given of the state of the neck and of the cord, to be perfectly correct in the opinion you have given?

" *Lord* Campbell.—Of course he does,—the question was put on that hypothesis.

" *Mr* Bodkin.—The statement is that the last coil of the rope was not so tight as the other two?—*Witness.* Yes.

" Have you heard it stated by any person that the three coils of rope were equally tight?—I heard one witness say so; but when I spoke about the tightness of the third coil I alluded to its being held firmly in the hand or fixed to a certain point.

" Supposing the person used both hands, would not the injury to the neck be greater?—If the party used both hands, and there was only one coil round the neck, there might be great injury to the neck produced, but if there were three coils the injury would be probably less, although sufficient to produce death.

"I believe the strength exerted in cases of determined suicide is very great frequently?—It is.

"Supposing a person determined on suicide to wind a small cord of this kind round the neck three times, and to pull it (*i. e.* one end of it) with both hands till insensibility was produced, and then fall, is that a state of things which, in your judgment, is likely to produce the injuries described?—I do not think that the violence found could be produced even if the person took both hands,—I do not think that with either one or both hands those injuries could have been produced. As I understand, the end of the cord was to the left, and the end of the cord must have been pulled in that direction, and consequently the right hand would not come much into play at all to exert any degree of force.

"Supposing the hand to grasp the cord tightly in the act of strangulation, is it not possible to relax it some hours after death?—Not where there is a tight convulsive grasp on the cord.

"Would the jar of the after-fall not have a tendency to make the hand let go of the cord?—I think there would be such an amount of power required to effect the injury by the twisting of the cord round the hand, that the cord would not be released from the hand by any accident of that sort.

"Is that an opinion theoretically given, or did you ever see such a case?—I only know of one instance of self-strangulation in this way, and then the cord was found in the hand.

"Have you ever known a case in which the grasp had been subjected to this test of the after-fall?—No, I do not know that I have.

"I think you said, if you had found the end of the cord once round the hand of the woman tightly, it would have made a difference in your opinion?—It might, because it would have had a fixed point to draw from; that is, if it had been firmly stretched.

"By firmly stretched, do you mean from the hand to the neck?—Yes.

"Would not a ligature of this sort cause the parts around it to swell?—It would, if tightly pressed.

"Would not that swelling present the appearance to a non-professional person of greater indentation than there really was?—As I understand Mr Williams to state there were two depressions, and the skin was greatly raised up between each; and there must have been great compression, as well as swelling, to force it up between the two.

" These questions of strangulation, I believe, are very difficult ones?—They are difficult, but not so difficult if we know the exact state of the body and cord; for then, I think, they may be distinguished. Suicidal might be easily mistaken for homicidal strangulation; homicidal strangulation cannot be easily mistaken for suicidal, because it is scarcely possible to place the body, or the cord round the neck in such a way as to deceive those who have attended to these subjects.

" In a case of decided homicidal strangulation, is it not easy to simulate suicide?—No, because in such cases too little or too much is done; in this case there was too much done. There may be cases in which it is difficult to distinguish between them, and there are others in which there is no difficulty; every case must be judged of by its own circumstances.

" In your book I find you refer to a celebrated case, which was pronounced by some to be suicidal and by others homicidal; there was a handkerchief tied round the neck so tightly that it was difficult to remove it. [He read the case.] I believe that gave rise to considerable discussion, and you in your book allude to it?—Yes: but in that case there was a knot to keep the handkerchief fixed, and the persons to whom it was referred formed an opinion that it was a case of suicide from the moral circumstances.

" In another case stated in your book, a woman was found lying on her stomach in her bed, and a woollen garter tied twice round her neck with two knots, and the circumstances of that case, you state, left no doubt that it was a case of suicidal strangulation?—No doubt of it; but the circumstances were widely different from those observed in this case.

" Would not the first knot of the ligature round her neck have had the effect you speak of in this case of depriving her of consciousness and power?—No, because the first knot, it appears, was put on the throat above the larynx, and the second knot was over the first; I imagine that the first constriction was not very tight, and was on that part of the neck where it allowed a certain degree of respiration.

" But it caused death?—It was sufficient to cause death, although probably not immediately, because death may be produced by any partial obstruction of respiration, as by a stocking tied round the throat.

" I will call your attention to another case here, that of a lunatic. The patient was found with his head hanging over the side of the bed; and when it was attempted to raise the body, it was found that respiration had ceased, and that the neck was constricted by a handkerchief folded up like a cravat; the first end of the handkerchief was exceedingly tightly tied with a knot, and then carried round and fastened by another knot. There was no doubt that this was a case of suicide ; but the reporter says if deceased had been found in a lone place, it would have been taken for a case of murder ?—Yes ; but the first turn of the handkerchief did not so compress the *windpipe* as to prevent respiration entirely, and it would therefore allow the possibility of a second turn and a knot in the same way.

" That fact does not appear in the report of the case ?—No ; but in both cases it is rather too loosely stated in the original reports where the pressure was. If it was above the larynx the person might breathe a minute or two; it is only where it is *on the windpipe* that we have that immediate and entire loss of power. I never tested it on myself; but an eminent Austrian practitioner has tested it on himself, and he finds that the effect materially depends on the part compressed by the ligature.

" I believe a few years ago it was generally received as an opinion that persons could not strangle themselves ?—Not generally, but only by those who had not studied the subject.

"Is there not another case where a person effected suicide with a stick forced into a handkerchief, and twisted round and fastened in his bosom ?—I have heard of it; but I am not prepared to admit that the first turn of the cravat or ligature was calculated to produce death immediately. It is a very common sort of suicide for persons to put a ligature loosely round the neck, then introduce something to tighten it, like a tourniquet, and so produce death; but that, in such cases, the first turn of the stick necessarily produces immediate insensibility or death, I am not prepared to admit.

" *Re-examined by Mr* James.—Must there not in this case have been a great deal of violence used to make blood flow from the nose, mouth, and nostrils of the deceased ?—I think so decidedly.

" Are you still, from the appearances of this body, of opinion that it was not an act of suicide ?—I am. The cases quoted are all cases distinguished from *this* ; there the ends of the ligatures were either tied in knots, and thus secured, or the ligatures were mechanically tightened by twisting with a stick.

" Do you give in this case a decided opinion ?—I do; that it was caused by another hand, and was not an act of suicide. There are cases in which there is no doubt, and I think this is one of them. It is an act of homicide by the violence of another."

The above extract contains a report of the whole of the medical evidence given at the trial. The part of my evidence enclosed in brackets refers to the prisoner. Although the manner in which he was connected with the charge has not yet been adverted to, I have thought it advisable not to disconnect this branch of the evidence from that which referred to the cause of death, as well as the important question whether the act of strangulation was homicidal or suicidal.

Having thus traced the state of the deceased from the time at which she was last seen living to the discovery of her dead body, its condition, and the proofs which it offered medically that she had died from violence by the hand of another, it will be proper to consider the circumstances by which the *prisoner* was connected with this alleged act of murder. It was distinctly proved that, for a considerable time, he had been in habits of close intimacy with the deceased. She was pregnant by him. It was reported he was about to be married to another person, while he had deceived the deceased by leading her to suppose that he would marry her; and the last fatal interview, it was presumed, was held with her under the pretence of making some positive arrangement on this subject. Hence her great anxiety to keep her appointment, leaving her meal half-finished. The prisoner had lately found her an incumbrance, and was evidently desirous, in some way or other, of shaking off the connection. Some time before her death, he had said to one witness, "he would like to shove her off, for he was afraid she would bring him into trouble." On the 29th of September, *i.e.*, fourteen days before her death, he caused her to write a letter affirming that her pregnancy was not by him. On the 1st of October, two days after this paper had been procured, while ploughing in a field, he told a witness that he had this letter in his possession, and said:—"You will hear of something very serious about Jael Denny: she told me on Sunday morning she would make away with herself." At this time he appeared confused, and did his work badly. When arrested on the charge of having committed the murder, he stated that he had a letter to show that the child with which deceased was pregnant was not his. He said to a witness voluntarily that he had heard her mother say she (the deceased) would destroy herself, and added:—the mother came to me, about fourteen days ago, and asked if my razors were all right; I said I would go and see, and I went and found them all right, and I asked what she asked such questions for;—she replied:—"I heard Jael say she would destroy herself, and I thought she would get at your razors." The evidence of the mother at the trial proved that the whole of this statement was untrue, and a pure invention on the part of the accused. Such is an outline of the *moral* evidence against the prisoner; and it was alleged by the prosecution to supply a sufficient *motive* for the act, and a desire to conceal it when perpetrated, under the pretence of suicide. The mode in which the cord was found lying near the left hand of the deceased also tallied with the view that a rude attempt had been made by the party who had perpetrated this crime to simulate the act of suicide.

The *general* and *medical* evidence further fixed the crime on the prisoner, by the fact that an appointment had been made by him to meet the deceased on the evening of her death; that his time was unaccounted for during the period which would have corresponded to the appointment and the time of her death; that, when taken, there were marks upon his clothing, proved by microscopical and chemical examination to be mammalian blood, while he referred the stains to a vegetable substance which could not possibly have produced them. These stains were fresh and still moist when he was first arrested; and an attempt had been made to remove, by wiping or rubbing, those which were more prominent, and in the front of his dress. Finally, the deceased had not been robbed or ravished; her body was found lying in a lonely spot in a field out of the high road, not far distant from the place of appointment made by the prisoner; and there was no reason for supposing that the hand of any stranger, or of any other person but the prisoner, was concerned in her death. The only marks of violence on her body were those which would have arisen from the mode of death, and the struggle for life during the act of strangulation. She had bled in *two* different places, her face was flattened by great pressure, and her bonnet crushed; the upper part of her dress was torn, while her lower garments were arranged in a smooth and orderly manner. All of these circumstances corroborated the medical inference, from the state of the cord round the neck, that this could *not* have been an act of suicide. It was therefore clear that either the prisoner, or some other person, must have committed the crime.

In cases of this kind, questions of time are of vital importance. For reasons to be hereafter stated, it was inferred, that, considering the state in which the body was found, the death of the deceased might have taken place soon after she left her house to meet the accused, or at a later period of the night, *i. e.*, from about seven o'clock in the evening up to half-past ten or eleven o'clock. The evidence for the prosecution brought out the following facts regarding the prisoner:—When asked by the mother, on the morning of the discovery of the body, where the deceased was, he said,—"I don't know, I have not seen her." The mother said:—"You did see her yesterday, between four and five o'clock, and talk to her, and appointed her to meet you at the stile near my house at half-past six; she went out to meet you, and where did you leave her?" He replied:—"I have not seen her *since five o'clock*. I went to Brentwood."

It was proved by the evidence of one witness, that deceased and prisoner were seen together on the Saturday evening at the stile at half-past five o'clock; they remained there about twenty minutes, which would bring the time to about ten minutes before six o'clock. The witness who deposed to the fact, left his work about six o'clock, and at that time received his weekly wages from the prisoner, who was then in his own house. He told the prisoner he was going to Brentwood, and the prisoner asked him to bring a saddle from that place for him, saying that he (the prisoner) should not go to Brentwood that night. This witness returned from Brentwood at ten o'clock that night and left the saddle at the kitchen door of the prisoner's house. He saw no more of the prisoner from the time his wages were paid; but it is to be inferred from the deposition of this witness, that the prisoner would have occupied another half hour in his usual duties. This would bring the time to *half-past six o'clock*, the hour of the appointment made with the deceased.

The next date is fixed by himself. When taken into custody, he asked a witness, named Hammond, what he was doing in his field so late on Saturday night? The witness Hammond did not see the prisoner, and would not have known that he was near the field, but for the question thus put. This field is situated between the spot where the body of the deceased was found and the town of Brentwood; and there is a road by the side of the field, along which the prisoner must have passed. This witness was in his field from *eight o'clock* to twenty minutes past eight. A little after *eight o'clock*, the prisoner was seen entering Brentwood with a basket of eggs, which soon after that time he delivered at the house of a person who had ordered them, and he remained in the house of this person about twenty minutes, when he went out into the town. The time was fixed by the prisoner himself, looking at his watch, and comparing it with the clock.

Evidence adduced in the defence for the purpose of establishing an *alibi*, only confirmed the testimony of these witnesses. A woman who acted as house-keeper to the prisoner, as well as her husband, deposed that he came home from his work between five and six o'clock on Saturday afternoon; that he had his tea, paid the men their wages, occupied himself in dressing, and that it was between *half-past six* and seven o'clock when he went out. The prisoner said he was going to Brentwood, and took with him a basket of eggs, which, in the course of the week, he said he should require on Saturday night. He took the key of the house with him, according to his usual practice. This witness went to bed at eight o'clock, and saw nothing more of the prisoner that night.

She got up the next morning about *six o'clock;* the prisoner got up about the same time, and she saw nothing unusual in his conduct. He brought some sugar and other articles from Brentwood, but she knew nothing of the hour of his return.

[It may be here remarked, in reference to the statements of these witnesses, and as showing what a deliberate purpose there was on the part of the prisoner to deceive all around him respecting the perpetration of this crime, that while he informed one witness he should not go to Brentwood that night, and had asked him to bring some articles from the place for him, he had, immediately after parting with this witness, dressed himself in order to go thither, taking with him a basket of eggs, which had been ordered for this purpose some days before. When he left his house to go to Brentwood, at or about half-past six o'clock, he went by the usual road which runs in a direction precisely opposite to the road leading to the stile where he had appointed to meet the deceased. Hence, when out of sight, he must have turned back for the purpose of keeping this appointment.]

Other witnesses called for the defence corroborated the evidence respecting the prisoner being near Hammond's field at or about *eight o'clock*, and he was seen by various persons in Brentwood up to *half-past nine o'clock* that night, after which there was no trace of him until *six o'clock* the following morning, when his housekeeper saw him.

Evidence was further called for the defence, to show that the *deceased*, some months previously, had talked of destroying herself; but the two witnesses did not agree about the date or the circumstances under which these conversations were held. Medical evidence was also brought forward to show that this might have been an act of *suicide* on the part of the deceased, and not a murder committed by the prisoner or any other person. I subjoin a report of the evidence given by two medical gentlemen who were called to support this line of defence.

"JOHN THORPE, surgeon of Maldon, *examined by* Mr BODKIN. —I have been a surgeon for twenty-seven years; I have had considerable experience; I have had occasion to inquire into cases of suicidal and homicidal strangulation. I was called in to give my opinion upon a case of suicidal strangulation lately, and that caused me to consider the subject of death by strangulation. The case I refer to occurred on the 10th of February last; I have heard the description given of the state in which the body in this case was found. From the facts of the case which I saw in February, and, assuming the description to be correct of the state of Jael Denny's body, I think *there is a doubt* as to whether she committed suicide or not.

"*Cross-examined by* Mr James.—I never saw a case till the one in February, and upon that I form my opinion. In that case there was no laceration of the neck, nor had the tongue protruded from the mouth. Both of these appearances must have been caused by violence, particularly the laceration of the neck by the coils of the cord; I agree with Dr Taylor, that the first coil drawn tightly round the windpipe would produce insensibility; the throat can endure greater compression at one part than another; at the upper part of the neck insensibility is not so soon produced as it is at the lower part; the rope was mechanically fastened to a stick in the case I attended in February, and the stick was holding the cord tightly. There is no analogy to *this* case in the mechanism by which the rope was held, and no analogy in the amount of violence. In my case the rope was only once round the neck, which by means of the stick was sufficiently tightened to cause death; from the ecchymosis on the neck, I consider the rope had been sufficiently tightened, and that the stick had got there by chance; that is, it had fallen from the hand into the man's waistcoat; there was considerable ecchymosis on the man's neck; it was the act of the deceased, and the hand of deceased was not upon the stick, which was fixed in the waistcoat.

"*By his Lordship.*—If I saw marks of great violence on a strangled body, it would make a difference in the opinion I should form.

"Thomas Pollock *examined by* Mr Bodkin.—I am a surgeon of Hatton Garden: my attention has lately been directed to homicide and suicide; I have had to examine a case of suicidal strangulation, which occurred in January last. Assuming the description given by the witnesses in this case to be correct, under the circumstances I should feel *considerable difficulty* in forming an opinion as to the cause of death, whether suicide or homicide.

"*Cross-examined by* Mr James.—Marks of violence on the body would be very material in aiding my judgment; flattening or pressure of the nose would also be very material, and these must be made by considerable violence.—I should agree with Professor Taylor that one coil drawn round the neck tightly would cause insensibility, and that the effusion of blood from the nose and mouth must have been produced by great violence; the laceration of the skin of the neck is also an indication of much violence, as is also the tongue protruding from the mouth.

"*Re-examined by* Mr Bodkin.—I have said that the marks just mentioned might have been caused by violence, and so they might by the fall of a heavy woman. From the appearances of violence found upon the body, they did not indicate greater violence than might have been committed by a person determined on suicide."

From what has been already stated, it will be perceived that the defence rested upon two points:—1st, the *possibility* of this having been an act of suicide; and, 2dly, if not, that the prisoner could not have perpetrated it, because the time and circumstances, as deposed to by the witnesses, could not be reconciled with such an hypothesis.

I here append some extracts from the summing up of the learned judge, in order to show the manner in which the medical facts were laid before the jury.

"On the part of the prosecution it is alleged, that this young man, having formed an illicit connection with the unfortunate girl Jael Denny, and she being with child by him, had formed the determination of making away with her in such manner that it should be suspected and believed that she died by her own hand, and that he might escape with impunity. They allege, that in the execution of that wicked design, he formed an engagement with her at half-past six, on the evening of the 12th of October, and that having met her, he perfected that design. On the other side it is stated that she committed suicide; at all events, if murdered, that there is not sufficient evidence to bring home that act to the prisoner at the bar. Now, you are first to consider whether Jael Denny was murdered; and, secondly, if she was, whether the evidence satisfactorily proves to you that Thomas Drory was the murderer. You are not to act on mere suspicion or probability. You must be morally convinced of his guilt before you find a verdict of guilty. There was no human eye saw the offence committed. It is what is called a case of circumstantial evidence; but if there is that evidence before you which is altogether inconsistent with the innocence of the prisoner, then you are to find him guilty just the same as if he was seen by a person to do it. You must consider morally as well as legally the evidence as to the probability of her having committed suicide, and on that point there was this circumstance to be attended to; that the deceased, being near her confinement, had provided for the midwife;—her mother was to nurse her; and you are to say whether under these circumstances she was likely to commit this act. And you will not only take into consideration moral but medical circumstances, assisted as you have been by gentlemen eminent in science; and here one thing was most important; that the end of the rope was loose between the fingers of the left hand; and it must be observed that when these great crimes are contemplated it often happens, by the dispensation of Divine Providence, they are committed in such a manner that they lead to their own detection. The father said when the body was found, there was blood about a foot from the side of the face, and how far that was to be explained on the theory of the deceased committing suicide, it is for you to say.

"On the morning of the 13th of October, when the mother said to the prisoner, 'You appointed to meet her at half-past six,' he did not deny it. It was for the jury to consider this. It was one of the most important points in this case; for if they thought there was that appointment, certainly the case assumed a different aspect. Spots of blood were discovered on the trowsers, and if they were spots of blood, it was very strange, that he should account for them by saying they were made by the stuff used for the calves. His Lordship then read through the evidence for the defence, and said he came at last to two witnesses, who, if they could believe them, would add materially to the supposition that deceased had committed suicide. Though their story was very extraordinary, if the wife had confirmed and corroborated the husband, it might have shaken their belief. They had heard what the wife said of it; the husband pledged his oath to the wife being present, but when they came to detail separately the conversation, respecting the intention of the deceased to destroy herself, it turned out to be totally different."

The jury returned a verdict of *guilty*.

Before his execution, the prisoner made a confession of his guilt, in which he stated that he met the deceased by appointment at the stile, about half-past six o'clock on Saturday evening; but in the meantime he had gone to a cellar in his father's house and taken from it part of a rope which was lying there. On meeting her for the second time, he said that he and deceased talked and walked about, after which, at her suggestion, they sat down on a bank. She had come to urge him to marry her. He passed the rope gently round her neck as they were sitting, and had got the end of it into a loop before she perceived it. She jumped up at once and put up her hands to save her throat, (at which time the marks of injury found upon them must have been inflicted,) but he pulled hard, and she fell without a struggle. He then left her lying in the field, and went to Brentwood.

Remarks.—In the above history of this remarkable case, I have not attempted to give a full report of the evidence of the witnesses, but rather an abstract of the principal facts proved at the trial, in order to throw light upon the medical questions on which the guilt or innocence of the prisoner depended. The medical evidence, however, on both sides, is given in full; and I am enabled, by the courtesy of the two medical gentlemen who appeared for the defence, to publish, in this paper, an account of the two cases which had led them to the belief that *this* might have been an act of suicide on the part of the deceased Jael Denny.

As the opinions of the medical witnesses for the prosecution were necessarily formed without reference to the moral circumstances, and in entire ignorance of many facts which were brought out by the trial and subsequent confession of the prisoner, it may be useful, for guidance on future occasions, to consider the grounds for opinions so strongly expressed. It is not often that in medico-legal cases, we can have a full confirmation of the accuracy of the views of medical witnesses, by a confession of the mode in which an act of murder has been perpetrated; and when this advantage is obtained, it is desirable to compare the actual facts with the data upon which the opinions were formed.

The medical evidence in this case resolves itself into five divisions:—

1. What was the cause of death?
2. When did death occur?
3. Was the act of strangulation homicidal or suicidal?
4. Within what period of time may death from strangulation take place?
5. Evidence affecting the accused, from stains on his clothes.

1. *What was the cause of death?*—The medical evidence proved, beyond all doubt, that the deceased had died from *strangulation.* She was found dead, with a cord firmly coiled round her neck. From the effusion of blood in two places, and the marks of violence about the neck, this cord had evidently been applied while she was living. The swelling and congestion of the face above the cord,—the distension of the eyes (or rather protrusion), the dilatation of the pupils, and the protrusion of the tongue, are all *external* signs of the mode of death. The distension of the *right* side of the heart with blood,—the congestion of the *lungs*, and the unnatural congestion of the substance of the *brain*, are the most marked *internal* appearances of death from strangulation; and these were all found in the body of the deceased. When to these proofs we add the general healthiness of the body, and the entire absence of any other cause to account for death, there is no other conclusion than that the deceased died from strangulation.

2. *When did death occur?*—The evidence showed that the deceased left her home, quite well, at *half-past six* o'clock, on Saturday, October 12th, and her dead body was found at *eight* o'clock on the Sunday morning. The body (*i. e.* the abdomen) was then *warm*, but the head, arms, and legs were cold, the latter rigid; and it has been already stated that *thirteen hours*

and a half had elapsed, since the deceased was last seen in perfect health.

The average period for the cooling of the dead body of an adult, would be about fifteen or sixteen hours, when exposed to the ordinary cool atmosphere of a room (at a temperature of about 60°), and the body is not much covered with clothing. In many cases, warmth is much longer retained. Within a recent period, I have had to investigate a case of death from suffocation by charcoal-vapour, in which, when the body was inspected twelve hours after death, it was warm all over,—the limbs still preserving their warmth. This person was found dead in a bed-room, in January, in which there had been no fire, and the body had lain loosely covered by the bed-clothes.

There was one circumstance which would tend to *accelerate* the cooling of the body in the case of the deceased, namely, exposure to the night air in the month of October. I have referred to the Report of the Registrar-General on the temperature of the date of October 12th, 1850. These thermometrical observations are made at Greenwich, but they may serve as a guide for the degree of cold in the country around London. The highest day-temperature, on October 12th, was 54°.5,—the lowest night-temperature was 37°.7,—mean temperature of October 12th, 41°.

In a temperature of 41°, the body would cool more rapidly than the average; but, in the case of the deceased, there were also circumstances which would tend to retard the process of cooling. She was a woman of good health and strength,—dying suddenly, in the prime of life,—dying from strangulation; and in death from asphyxia, the cooling of the body generally takes place slowly. She was pregnant;—her body was covered with clothes, and, when found, it was in a sheltered spot, near a hedge. These circumstances would tend to preserve the heat of the great cavities, such as the chest and abdomen, which are the last parts of the body to lose heat; and therefore the perception of warmth in the body of the deceased when found, is quite compatible with her having died at from half-past six to seven o'clock on the evening of Saturday, October 12th.

There is, at the same time, so much uncertainty about the rate of cooling of the dead body, that the time of death cannot be fixed with any precision by the presence of this residuary warmth in the abdomen. It must, therefore, be conceded that the deceased might have died *at any time between half-past six and twelve o'clock*, on the night of Saturday, October 12th. Had she died later than this, *i. e.* at any time in the morning of Sunday, October 13th, it is probable that a great degree of warmth over the chest and shoulders, as well as over the whole body, would have been discovered.

[This inquiry, respecting the precise time of death, was not entered into at the trial. At the inquest, the following questions were put respecting the cooling of the dead body:—

"*The* CORONER. In your opinion, how long may a body retain heat after death?

"*Dr* TAYLOR. I have known it, as a matter of medical experience, that heat has been retained as long as thirty or forty hours—that is to say, in the trunk of the body, not in the extremities.

"*The* CORONER. In ordinary cases, what opinion should you form as to the time a party had been dead from the extremities being cold and rigid?

"*Dr* TAYLOR. There are many circumstances to be noticed —the age of the party, quantity of clothing, &c.; but it is a fact, that where persons die a violent death in full health and vigour, their bodies are much more slow in cooling than under ordinary circumstances. The average time of cooling after death is from fourteen to sixteen hours.

"*The* CORONER. In your opinion and judgment would the circumstance of a woman being in the ninth month of her pregnancy tend to retain the heat longer?

"*Dr* TAYLOR. It would, for this reason, that it would give greater bulk to the body.

"CORONER.—Supposing this poor woman to have been murdered at half-past six o'clock on the Saturday night, would there be anything extraordinary in finding heat in the body at eight o'clock the next morning?

"*Dr* TAYLOR. If the body was well clothed I should say it is quite possible for the trunk to retain warmth for that period of time."

The evidence given at the trial, with the statement made by the prisoner subsequently to conviction, shows that the deceased must have been strangled about seven o'clock. Her body had, therefore, been exposed during thirteen hours, a result quite in accordance with medical experience, and with the medical opinion given before the facts regarding time came out in evidence.]

3. *Was the act of strangulation homicidal or suicidal?*—This was undoubtedly the most important medical question in the case, and it is one upon which, as it will be perceived by the report of the trial, there was a difference of opinion between the two medical witnesses called respectively for the prosecution and

defence. While the two witnesses for the prosecution maintained that the act was *not* one of *suicide*, but of homicide; those for the defence went no farther than to assert,—the one that he thought there was "*a doubt;*" the other, that he should have "considerable difficulty in forming an opinion whether it was an act of suicide or homicide." Thus, then, the witnesses for the defence could not say that in their judgment this was certainly an act of suicide. These opinions could hardly be said to be conflicting, because the witnesses for the defence merely declined giving a decided opinion one way or the other. Doubts and difficulties exist in nearly all cases in which medical opinions are required; they can only be resolved and removed by a most minute inquiry into facts; and, if witnesses have not had an opportunity of making such an inquiry, it is not be expected that they can speak with decision upon an abstruse subject like that involved in this investigation. The cases which led these gentlemen to entertain doubts in reference to the act whereby Jael Denny lost her life, will, I think, be found to be easily explicable upon a comparison of the circumstances. As it will be seen hereafter, there was nothing in these cases incompatible with suicide; but there was everything incompatible with suicide in the case of Jael Denny, even supposing that no one had been charged with the murder. This, as it appears to me, is the true way in which to consider the question; for it is obvious that a medical inference from a series of appearances found on a dead body, ought not to depend upon, or to be modified by the fact of a person being charged with murder. An accidental circumstance of this kind may properly give rise to very great caution in drawing an inference; but it should not be allowed to have any other influence on a medical opinion. It is unnecessary to recapitulate the evidence given at the trial; but it may be proper to state in this place the series of facts and inferences which led me to give a very decided opinion, that this could not have been an act of suicide.

1. The deceased was *right*-handed; and on the hypothesis of suicide, she must have made the tension with her *left* arm and hand. From the position of the loop or noose, any traction to the right would not have tightened but have loosened the cord.

2. That, supposing her to have exerted such a traction at all she must have been in the erect or sitting posture. The force used, indicated by the great local violence to the neck, could not have been exerted by a person attempting to tighten a cord by drawing it to the left while in a recumbent posture, whether prone or supine.

This hypothesis would, besides, leave wholly unexplained the flattening of the nose, (obviously from direct pressure, not from a fall,) and the fact that deceased had bled in two places, one spot being a foot from the other.

3. That the cord must have been pulled with excessive violence in a horizontal direction by one end only, as the mark was circular round the neck.

The other end of the cord formed a noose or loop, and was tightly fixed at the back of the neck. Thus, then, all the force of traction must have been exerted to the left, in which direction the right hand of a right-handed person could not act horizontally, so as to produce the amount of violence found on the soft parts of the neck.

4. That the fact of there being three coils and a half of rope round the neck, formed an obstacle to the tightening of the cord by pulling one end to the left so as to imbed the two inner coils in the skin, and to leave the outer or third coil loose.

On the supposition that the deceased produced the constriction by her own act, it follows that the three coils must have been round the neck at one time, and the two inner coils sufficiently loose to allow of respiration before traction was commenced.

5. The double indentation found on the trachea, could not have been produced by the two inner coils (on the supposition of suicide), except by the great tightening of the outer coil.

6. As insensibility and loss of power must have immediately followed the complete compression and obliteration of the trachea by the two inner coils, the outer coil ought not to have been found loose or unconnected with the object by which the force of constriction had been produced.

To suppose that the deceased could have produced the intense constriction by the first coil, and have afterwards retained sufficent power to pass a second coil from right to left around her neck, indenting the skin, and flattening the trachea as much by the second as by the first coil, involves, in my judgment, a physiological impossibility. There was, therefore, on the suicidal hypothesis, no explanation to resort to,—but that all the three coils had been placed at once round the neck loosely,—that one end only of the cord had then been so pulled to the left as to produce the great amount of violence found, and to tighten equally the two inner coils; while the outer coil and extremity of the cord, by which this immense force must have been applied to the two inner coils, was found lying loosely, without any attachment either to the hand of the deceased or to any other fixed point.

7. To have indented the neck, compressed and bruised the trachea in two distinct places; to have caused effusion of blood to the amount of a cupful from mouth, nose, and ears,—this effusion being found in two distinct places a foot distant from

each other,—would have required a very considerable tension of the outer coil, and, at the same time, a *continued* tension,—lasting sufficiently long for the head to move a foot after a cupful of blood had been lost as a mechanical result of the first constriction.

8. Admitting such conditions of the body and cord to be compatible with suicide, the act could only be conceived to be possible in this case, by the fact of the end of the cord being found tightly wound round the left hand of the deceased.

9. On the suicidal hypothesis, it would have undoubtedly required a very firm grasp of a rope to produce such effects as were here observed; and from the rapid production of unconsciousness by the compression of the trachea and the arrest of respiration, it would have been impossible, on the part of the deceased, to relax the grasp. Hence the cord should have been found either firmly held in the hand in the rigidity of death, or wound round it in a state of tension.

Unless we adopt this view, we must suppose that after having used an enormous amount of violence by a rope in the left hand, the dead body had the power of relaxing the grasp,—of loosening the outer coil of cord, and so moving the hand that the end of the cord should be found lying between the finger and thumb and barely touching the palm.

Such a condition is not only physiologically, but in this case, as it will be presently shown from the length of the cord, physically impossible.

10. Admitting the improbable supposition that the end of the cord had been wound round the hand of the deceased, or grasped by it with the necessary firmness to cause laceration of the soft parts of the neck, and an effusion of blood from mouth, ears, and nose,—and that it had fallen from the hand subsequently to death by a spontaneous relaxation of the muscles, it is reasonable to expect that the soft parts of the hand would present some indication of such a force of traction of having been employed by the deceased; but the palms of the hands and fingers presented no appearance whatever of such an extraordinary exertion of muscular power having been made by the deceased. There were marks of violence at the *back* of the hands and on the arms out of the course of the cord; but these were of such a nature and in such a situation, that they could not have have been, by any possibility, produced by it.

11. The *length* of the cord renders it impossible to suppose that such a force could have been exerted by the deceased herself. I ascertained, by measurement, that its total length was *fifty-nine* and *a-half* inches. It has been already stated, that there were three and half coils round the neck, the first quite

tight and close to the skin; the second also tight, but enclosing part of the apron of the bonnet, neckerchief, and cloak; and the third more loose than the other two. Making due allowance for these circumstances, and the fact that the circumference of the lower part of the throat of a full grown woman like the deceased, cannot be taken at less than *fifteen* inches, the three coils and a half would have consumed fifty-two and a half inches of the cord, leaving only *seven inches* for the traction! This was barely enough to reach the finger and thumb of the raised left hand, and not enough to allow of such a firm grasp by the hand as would be necessary to the production of so much violence to the soft parts of the neck. I find, by measurement, that the circumference of a small female hand in the adult is rather more than *seven inches.* This measurement includes the palm of the hand only without the thumb, and embraces that part of the hand around which a coil would be placed when the object of a person was to produce firm traction.

Hence, then, the hypothesis of suicide involves one of these physical conditions. Without a firm hold of the cord, which could not have been had with less than one coil round the hand, it is impossible to conceive that such violence to the neck could have been produced by the act of the deceased; and if one coil had thus been spontaneously wound round the hand, it would have consumed the whole length of the cord up to the last half coil, and left no portion whatever to give a purchase for pulling with so much violence. Either condition is a physical impossibility; and no theory will suit the facts or explain them, excepting that which admits that the act was not the result of suicide, but of manual violence applied by another person.

The opinion which I gave before the trial and confession of the prisoner, from a consideration of the circumstances abovementioned respecting the mode in which strangulation was accomplished, is thus expressed on my notes: "1. My belief is, that after the first coil was tightened, more force was employed with the second coil than the deceased herself could have used. She would have been insensible and powerless. 2. To produce by spontaneous exertion, by a cord passed round the neck without any fixed point to hold it, an effusion of a quantity of blood from the mouth and nose, requires a continued application of force beyond the power of a person whose windpipe is already so compressed and flattened as completely to obstruct respiration."*

It must be obvious from the foregoing considerations, that the statement of the learned judge (p. 78) is perfectly borne out by the facts of this case when properly interpreted by the aid of physiological and physical science:—"When great crimes are contemplated, it often happens, by the dispensation of Providence, that they are committed in such a manner that they lead to their own detection." The fact is, in all these cases, the murderer who attempts to simulate suicide by this mode of death, either does too little or too much. In this instance, as the learned counsel for the prosecution remarked in a very eloquent reply on the part of the Crown, "too much was done!" The rope was too tightly drawn,—it was placed in too many coils round the neck,—it was not long enough to allow of the hold necessary for the deceased to exert the necessary force,—it took the wrong direction,—the force being exerted to the *left* and from behind in a person who was *right*-handed,—the deceased had bled in two distinct places, when, considering the source of the blood, this could not have been effused in either spot without the production of insensibility, and a perfect loss of all muscular power,—so that the bleeding in the second spot in which the nose was found flattened, and the bonnet crushed on the soil, clearly indicated, that after the first act of complete strangulation, the head must have been moved; the end of the cord was lying loosely, and not convulsively grasped in the dead hand; the upper part of the dress was in great disorder, torn, and dirty, the lower part had been smoothed down, and was arranged in an orderly manner. These circumstances conclusively show that, when a murderer attempts to simulate an act of suicide under such a mode of death, the attempt must end in failure. The more he strives to imitate the act, the more likely is he to multiply the sources of circumstantial evidence against himself. For this reason, in replies to questions on this subject at the trial, I stated to the learned counsel for the defence, that suicidal might be easily mistaken for homicidal strangulation; but that homicidal could not be mistaken for suicidal strangulation when the body was not disturbed and a proper and minute investigation of all the circumstances was made. Too much or too little is done. The most accomplished assassin would, I believe, in such an attempt, utterly fail too meet all those requirements which a proper knowledge of physiology and physics would find to be essential on the hypothesis of suicide. There is greater difficulty here than, perhaps, in most other forms of violent death; because suicidal strangulation is comparatively rare, and requires special means and preparation for its accomplishment. Doubts and difficulties will, of course, exist in these cases, and create differences of opinion. The degrees of doubt or certainty in medical opinions, depend entirely upon degrees of observation, experience, and upon extent of previous study and reflection. Hence, one who has only a half knowledge of the facts, or leaves some which are important, although minute, out of his consideration, cannot be in a position to form an opinion entirely free from doubt. In no other way can I explain the fact that a good anatomist and physiologist suggested to me a few days before the trial, that this act might have been one of suicide. This opinion was formed upon a comparison of some few reported cases, in which the circumstances, when strictly analysed, were so dissimilar as to justify no analogical application, except in the bare fact that death was caused by strangulation. It was undisputed that strangulation might be an act of suicide, but it was positively denied that, in this case, the act was suicidal.

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* There is no doubt that the deceased was strangled by the first coil;—the second was made immediately to ensure death;—while the third was put round the neck loosely, in the attempt to assimilate suicide.

The cases upon which Mr Thorpe and Mr Pollock, the witnesses for the defence, felt themselves justified in stating that the strangulation of Jael Denny might have been suicidal, are curious and instructive. These gentlemen have kindly favoured me with the following particulars of their cases, which I give in their own words.

Mr THORPE's *Case.*—" C. H—, æt. 58, left his home at 7 A.M., for the purpose, as he stated, of going to London to present a bill. On his return home he effected self-destruction in the following manner; he passed a noose of cord over his head, and then inserted a stick, about fourteen inches long, between the cord and his neck. Having done so, he, with the assistance of the stick, twisted the end sufficiently tight so as to cause, I should say, almost immediate suffocation (strangulation ?) Still it appeared there was time for him to insert the lower end of the stick in the inner side of the waistcoat, and the upper end was accurately adapted to the internal jugular vein and carotid artery. The date of this suicide was the 11th of February." (1851.)

In this case, it will be seen, the deceased committed suicide by employing a circular ligature and tightening it with a stick, on the principle of the tourniquet. The cord was tightened by the twisting of the stick sufficiently to obstruct respiration, and the stick was retained in the inside of the waistcoat in such a position as to prevent it from slipping or loosening the cord. Here, then, was a fixed point found for the constriction, and sufficient to explain it. There was nothing in the position of the stick or the cord incompatible with the fact of the man having perpetrated the act himself. There was no more violence to the neck than the stick and cord could have produced by the deceased's own act. There was no effusion of blood from nose, mouth, and ears. The man was not found with his dress torn, his nose flattened, and other marks of recent violence on his person. Had these appearances existed on the body, and had the stick been found loose or lying at a distance, with no fixed point from which the constriction could have originated, it is probable that the opinion of suicide would not have been entertained in this instance, or at any rate considerable doubt would have arisen whether it might not have been an act of homicide. As it is, the case merely proves that a person may strangle himself by the aid of a noose and a stick; and there are several such instances on record. In no one point, except in the cause of death being the same, does it bear any analogy to the case of Jael Denny.

Mr POLLOCK'S *Case.*—"Pizzalla, an Italian, about fifty years of age, employed as a porter, was found dead in the forenoon of the 3d January 1851, in an attic of the house of his employer. He had been missing from his employment thirty hours. When found he was lying on his back, rather inclining to the left side, with a piece of ordinary sash-line coiled four times around his neck, two of the coils so tight and imbedded therein that there was some difficulty in undoing it. The right hand held one end of the line, and the left hand the other, with a turn of the line around each to hold it the more securely. The right arm was extended, the left flexed. I made a *post-mortem* examination of the body on the fourth day after it was found. Externally, the face was swollen and purple, the vessels of the conjunctivæ were congested, the tongue protruded towards the left side, bloody froth issued from the mouth, and the lower jaw was slightly twisted towards the left side. The skin of the neck was abraded in a nearly continuous line around it, about five-eighths of an inch in width, and presenting the appearance of being produced by two coils of the line. There was considerable ecchymosis above and below the line of abrasion. Each hand retained the impression of the line being coiled around it. Internally, the vessels of the brain and its membranes were greatly congested.

"The evidence before the coroner left no doubt of this having been a suicidal act."

This case proves that a person may strangle himself and that he may accomplish strangulation by pulling the two ends of a cord coiled several times round the neck; and that some degree of local violence to the neck may thus be produced by the ligature used. These are points which could not reasonably be disputed, even supposing that no such case as that of Pizzala had occurred. In what way, however, would the facts of this case justify a medical opinion that the deceased, Jael Denny, had destroyed herself? She was on her face, with her nose flattened and other marks of violence on her person. The cord was not held in or wound round each hand, and there was no mark or impression to indicate that it had ever been coiled around either hand. In Denny's case blood had been forced out of mouth, nose, and ears in two places, and the amount of local violence was much greater than in that of Pizzala. It is clear, then, that in the left hand of Denny, if this had used the force, a much stronger impression should have been found than in either hand of Pizzala. In the case of Denny the cord was not long enough to form even a coil round one hand, while in that of Pizzala it was long enough to form a turn round both hands, the right arm being extended. In Pizzala's case both ends were strongly pulled to tighten the coils; in Denny's case there was only one end which could be pulled, and this was of insufficient length to allow of that amount of traction even with one hand which would correspond to the local violence!

There is, therefore, no parallel between the two cases, except in the mode of death, and the means used to strangle. Had the facts regarding the body and the cord in Denny's case been the same as that in Pizzala's, no medical witness would have been justified in asserting that the act was one of homicide. It was simply because the facts were *not* the same, that a decided opinion was given by the witnesses for the prosecution, that the act could not have been suicidal. I have met with such a case as that of Pizzala ; one coil of the cord only being round the neck, and the two ends being firmly wound round both hands. The person was found dead with the ends of the cord in both hands. I do not know that we can possibly have any better indication of suicide than this; for no murderer who has used a cord to strangle another, would be able by winding the ends round each hand of the dead body, to imitate the tight convulsive grasp (passing from muscular contraction into cadaverous rigidity), by which alone the act of suicide under such circumstances could be accomplished. If he turned the cord around each hand, how is he to imitate or produce the impression which is left by the force used (in suicide), on the living hand? The existence of these impressions led justly to the inference of suicide in the case of Pizzala; their absence, with the other circumstances mentioned,—to the inference of homicide in the case of Jael Denny.

Among cases that have been communicated to me, I find the following which occurred in 1836. A woman who had been for some time past in a distressed state of mind, was found dead in her bed with one of her stockings tied round her neck. It appeared from the evidence that she had been left alone for a short time, and that she must have taken this opportunity of strangling herself by tying the stocking round her throat. It was found to be rather tightly drawn, but in a single tie. There was a distinct mark round the neck where the constriction had been applied; and the examination of the body left no doubt that the deceased had died from strangulation. With the exception of the mark on the neck, there were no traces of violence on the person.

In this case the roughness and breadth of the stocking were sufficient to hold it to the neck by a single tie, when it had once been drawn tightly by muscular exertion. In such a mode of suicide the tie might be suddenly made and the hands withdrawn; but the position of the hands is not described. But little force would be required to compress the trachea so as to obstruct respiration and circulation; and the ligature, from its nature, would not leave any marks or impressions of violence on the hands.

It will be observed, from the report of the evidence (*ante* p. 379), that counsel for the defence referred to certain cases of suicidal strangulation, in which, as he supposed, the facts were such as to justify the inference that the deceased, Jael Denny, had destroyed herself. I subjoin an abstract of these cases, as on a future occasion they may be of service to gentlemen, who may be required to give evidence in a case of alleged murder by strangulation.

The first case, alluded to by the learned counsel, is reported in the Annales d'Hygiéne, for 1829, vol. ii. p. 447. The deceased, who was a servant girl far advanced in pregnancy, was one morning found dead in her bed. The body was cold and rigid. A common pocket-handkerchief was found round her neck, tied in a knot so tightly, it is said, as to render it difficult to remove it. The knot was on the left side of the neck,—but the deceased was right-handed. On a closer examination, it was found that there were two knots: and there was the mark of a necklace, worn by the deceased, on the skin of the neck. With respect to the attitude of the body when found, it was lying on the bed with the face turned to the right,—the right arm was raised by the side of the head, and the left lay across the upper part of the abdomen. On removing the handkerchief, a quantity of bloody mucus escaped from the mouth and nostrils. The neck was tumefied as if from pressure, but there was no ecchymosis in the course of the ligature. There was no trace of violence on any part of the body.

A suspicion of homicide arose in the case: —1*st*, because there had been no apparent motive for suicide; 2*d*, because the knot was on the left side of the neck, while the deceased was right-handed; and, 3*d*, because the handkerchief was tied in two knots, and it was supposed that deceased could not have retained the power of tying a second knot if she had really tied the first.

The first two points require no consideration, as they prove nothing medically or morally in favour of homicide. As to the third, it may be stated there is nothing to prevent a person tying two knots in a handkerchief, provided the ligature be not drawn so tightly round the lower part of the neck as to compress and obliterate the trachea. No ecchymosed mark had been produced on the neck; a fact which shows that the constriction could not have been at any time very great, and the part of the neck, included in the ligature, is not stated. As the ligature was secured by knots, it did not require tension by the hands. There was no violence on the body, and there was nothing in the case incompatible with the act of suicide. Except that the deceased had died from strangulation, there was no analogy whatever in the circumstances to the case of Denny. This is substantially stated at p. 717 of the work from which the learned counsel quoted.

In the second case, reported in the Annales d'Hygiène, 1829, vol. ii., p. 440, the deceased, a female, was found lying in bed on her face, and one of her garters passed twice round the middle portion of her neck, and secured by two knots in front (*i. e.*, one knot tied strongly upon the other). There was only a slight or superficial mark produced by the ligature, rendered apparent by a few ecchymosed spots. This was perceptible in the fore part, but scarcely visible behind. The face was swollen and livid, and the tongue strongly clenched between the jaws; a sanguineous mucus escaped from the mouth and nostrils. There was no disorder of the dress, bed, or furniture, and no mark of violence on the person. On dissection, there was no subcutaneous effusion, and no violence to the muscles between the skin and the larynx. The larynx itself, and the upper rings of the trachea presented no marks of injury. The examiners came to the conclusion that this was an act of suicide.

It is obvious, from the description, that the seat of pressure here was high up, and embraced the lower part of the larynx. The constriction was not sufficient to produce a deep mark, and those who inspected the body expressly state, that, although the two knots were well secured, the first ligature itself had only been drawn with moderate force (*mediocrement serrè*) round the neck, so that, in their judgment, it had caused death by interfering with the cerebral circulation only, producing apoplexy, and not by suddenly interrupting respiration and producing asphyxia. Under the circumstances in which the body was found, considering the seat of pressure, and the small amount of pressure as determined by the dissection, it was quite possible for the deceased to have tied two or even three knots in the garter. The circumstances in Denny's case were so entirely different, as not to admit of any medical comparison with those recorded in the case of this female.

In the third case, reported by M. Rendu (Annales d'Hygiène, 1833, vol. ii., p. 152), a female lunatic was found dead in her bed, with a folded neck handkerchief passed round her neck. The first turn, which was rather close, had been produced by bringing the handkerchief from behind forwards,—a plain knot or tie had then been made in front, the ends carried backwards round the neck, again brought forward and secured by another simple knot in front. There was ecchymosis on the front and side of the neck. This was undoubtedly an act of suicide. There was not a single fact to create even a suspicion of homicide, except that having lost four fingers of her right hand, the deceased had very little power over it, and it might have been a question whether, with a maimed hand, she could have folded and tied the knots in the manner in which they were found.

Neither the part of the neck here compressed nor the amount of violence caused, is mentioned in the report. We cannot infer from the report that the first turn of the handkerchief was so tight as completely to arrest respiration and prevent circulation, and in failure of evidence of these results, the whole case resolved itself into the following question:—Could a female, who had lost four fingers of one hand, pass a cravat round her neck, and tie two simple knots in the manner in which they were found? Even had the fact of suicide not been quite clear in this instance, no medical witness would have been justified in saying that such an act was impossible or even improbable. Analogy here also fails. The deceased Denny had the use of all the fingers of both hands, the rope had not been tied in one or two knots, and instead of a loose compression of the neck generally, there had been a tight constriction of the trachea in two distinct places.

Other cases might be cited to show that suicide by strangulation can be perpetrated under remarkable and even unexpected circumstances. This, however, was not the question at issue. In every case, adduced by the learned counsel, there was no proof that the trachea had been so tightly constricted by the first turn or coil of a ligature, as to render it probable the individual would have lost all power to make a second turn. A person who did not completely arrest respiration by the first turn, might tie a knot or make a second turn with great facility. In all the cases quoted as similar to that of Denny, there was this marked dissimilarity,—the ligature was tied, secured, or fixed, so that it could not slip. Even in Mr Pollock's case, the two ends were held tightly.

4. Within what period of time may death from strangulation take place?—Death from strangulation, when the trachea is so

compressed that the tube is flattened, may take place in a few minutes. Insensibility, loss of consciousness and power to make any muscular exertion, cease almost immediately; hence, it was strongly alleged in this case, that after one constriction of the neck had been made in the degree proved by the amount of violence, a second constriction could not be made by the individual; he would lose the power and consciousness necessary for such a muscular exertion. Both the witnesses for the prosecution and defence concurred in the view, that insensibility would be an immediate result of a coil of rope being tightly drawn around the windpipe; and all agreed that when the pressure was at the upper part of the throat above the windpipe, insensibility is not so soon produced as it is when the pressure is at the lower part. In fact, it is generally admitted by physiologists, that the throat can bear greater or longer continued compression at one part, without producing unconsciousness, than at another. Above the larynx, the mechanical disposition of the muscles and of the parts about the fauces prevents that direct stoppage of the act of respiration, which would immediately follow when the same degree of constricting force was applied to the trachea.

On this subject, the experiments of Fleischmann made upon himself are of great interest. It was to them I referred in my evidence. This gentleman found that when he placed a cord round the neck, in the space just below the chin, it might be strongly drawn, either laterally or behind, without the act of respiration becoming perceptibly disturbed, and a person may, under these circumstances, inspire and expire for a long time. This is what might have been anticipated, since a ligature so placed nowhere compresses the air-passages. But under this constriction, the face becomes red, and the eyes congested and protruding; the head feels hot, there is a sense of weight, followed by vertigo, and there is suddenly a hissing noise in the ears. This last is a symptom of impending danger; and unless the experiment be discontinued at this time, the result may be fatal. These effects show an interruption to the return of blood from the head.

Similar effects are produced when the cord is applied over the larynx. In this case the symptoms come on with greater rapidity, and there is from the first some difficulty in breathing. Th previous experiments could be carried on for two minutes; bu in this case, scarcely thirty seconds had elapsed before the hissin noise in the ears came on, and with this an indescribable sens tion in the brain. The variation in the seat of pressure, explai the difference in the latter case; there would be a greater n chanical interference with the act of respiration, and a grea obstruction to the return of blood from the head.

When the pressure is over the thyroid or cricoid cartilage, there may be a slight degree of respiration. When it is below the cricoid and over the trachea, the interruption to respiration is instantaneous, and the pressure can be borne only for a very short period.* The effect of a complete obliteration of the lower part of the trachea by a ligature, and consequent constriction of the neck, was not tried by Fleischmann, as from the effects produced by slight pressure at the upper part of the trachea in connection with the larynx, such an experiment might have proved fatal.

These experiments clearly prove, that the power of continuing to respire, without which, in some degree, acts of volition, consciousness, and muscular exertion cannot be performed, depends on two circumstances:—1. The precise seat of compression, and 2. The amount of compression. It is worthy of remark, that, according to Fleischmann's previous observations, if the compression should be immediately over the os hyoides at a time when the person is in the act of expiring, the dangerous effects come on very rapidly,—obviously from a closure of the glottis.

Dr Marshall Hall has lately shown by experiments, that an impeded flow of blood from the head along the veins of the neck is instantly followed by insensibility; and he considers that the first effect of a cord tightened round the neck, in the manner described in the case of Denny, is the same as that of trachelismus in the apoplectic state. Such a constriction must, in his opinion, have produced immediate insensibility. This influence of interrupted cerebral circulation has probably not yet been sufficiently attended to by pathologists. Dr Hall found that even when the trachea of a dog was kept open by a tracheotomy tube, a cord tightened round the upper part of the neck produced a momentary struggle, then perfect insensibility and immobility, with a loss of all muscular power. He would, therefore, refer the effects rather to the degree than the seat of constriction. His experiments clearly show that insensibility and unconsciousness may be the immediate results of a constriction which obstructs the cerebral circulation alone, although it may not entirely impede respiration. In homicidal and suicidal strangulation one condition cannot be produced without the other.†

Dr Kay, who is inclined to attach more importance to the entire exclusion of air from the lungs than to the prevention of the return of blood from the brain, says, If air be excluded a short interval only elapses ere vertigo and cerebral oppression ensue, followed by a torpor which benumbs every sense. The face and lips become livid, and the surface generally more dusky than the natural hue. The pulse is rapidly enfeebled, and is sooner extinct,—first in the extremities, and afterwards in parts nearer the centre of circulation." (On Asphyxia, p. 70.)

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* Devergie Médecine Légale, vol, ii, p, 370,

† Croonian Lectures, 1851,

These facts regarding the effects of even a moderate compression of the air-passages in rendering a person rapidly insensible and powerless serve to explain how it happens that, in numerous instances of suicide by hanging or strangulation, the bodies are found kneeling, lying, or otherwise supported in a position in which the suicides might be supposed fully capable of moving, and thus of saving themselves. But insensibility is most insidiously and most rapidly induced, and a partial circulation of imperfectly arterialised blood suffices to prevent not only muscular exertion, but even the consciousness of the necessity of making it. Dr Jaquemain examined a case of suicidal strangulation in which the deceased had passed a cord round his neck, and secured the other end to a nail in the wall a little above his bed.* When the body was found, it was partly inclined against the wall in a kneeling attitude. The two feet rested on the bed. The constriction in this instance was on the neck generally, and it was only partial, as the weight of the body was, in a great degree, supported. In a case which occurred to M. Esquirol, a female lunatic passed a cord round her neck, fastening the other end to a short post on an embankment. She had then allowed her body to slide downwards. Although seen to commit the act, and assistance was instantly rendered, she was found to be quite dead by the time the attendants reached the spot. Instances of this kind might be multiplied, but these will suffice to show that insensibility and powerlessness to move or alter a position, are very rapid effects of a general compression of the neck when respiration is even only partially impeded.

In hanging by the whole weight of the body, the line of constriction is generally above the larynx, owing to the slipping of the cord; hence the pressure on the air-passages is not so complete as in strangulation by close constriction of the trachea. Nevertheless an individual is speedily rendered insensible and powerless even under these circumstances, doubtless from interruption of the return of blood from the head. I am indebted to Mr B. W. Richardson, of Mortlake, for the following singular case, in which the individual committed suicide by a mixed condition of hanging and strangulation.

"On the 2d of May, 1851, between 7 and 8 o'clock a.m., Mr Beresford was summoned to the village of Tubbesthorpe, near Leicester, to see a farm-servant who had hanged himself. On arrival he learnt that the occurrence had taken place in a barn, and found the man supported in the sitting posture, and not dead, but insensible, and breathing slowly, and in a sighing manner, with now and then slight stertor. The face was livid; pupils dilated, but acting freely to the stimulus of light; pulse 80, small; general surface of body cool; extremities cold; there was no frothing of mouth; the mark of the ligature was most distinct in a pink line.

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* 407 Ann. d'Hygiène, 1831, i. 202.

"On inquiring into the mode of suspension, it was found that the body had most probably been hanging a considerable time,—the man having been missed for more than twenty minutes. The ligature he had used was a thick waggon rope. He had made a noose in the middle of it for the neck, and then, throwing the two ends over a high beam, had got upon a stool, and fastened one of the ends of the rope to one of his wrists, by a close knot, grasping the other end higher up with the remaining hand, and so suspending himself by spring off the stool. In this condition he was found; the weight of the body being shared by the wrist, hand, and neck. He was at once cut down; but in their fright, the persons around made no provision for his fall, and so he came heavily down upon the floor. At first he was thought to be quite dead; but after a little time he showed some signs of life, and continued to rally until Mr Beresford saw him.

"Mr Beresford attempted to take blood from the jugular veins, but failing, had recourse to the veins at the bend of the arm, from which he got a pint without difficulty; the effect was for the time most favourable; the stertor ceased, the pupils dilated less, and the face assumed a natural colour and expression. This continued for half an hour, during which time the patient was placed comfortably in a warm bed, and had mustered sinapisms to his feet, &c. After the lapse of this period he began again to grow worse, the stertorous breathing returned, and with it strong convulsions came on. Two pints more blood were now taken, but made no change in the symptoms, except that of inducing profuse perspiration; other means were resorted to, but without avail, and the man died about seven hours afterwards in a violent convulsive fit.

"Mr Beresford remarks that the convulsions, at times, quite amounted to opisthotonos; and that, as death approached, the breathing became gradually slower and seemed to be attended with great pain, the man groaning heavily with each expiration.

"The number of inspirations per minute was 44 after the second venesection, but eventually they fell to 40. The pulse became, towards the end, more and more rapid and irregular, at one time being, at least, 200 per minute. The pupils were dilated widely towards the last; but if a light were brought near them, they momentarily contracted, and then returned to their full dilatation, although the stimulus remained. A post-mortem examination was most unfortunately not made. I should just add that Mr Beresford and another gentleman who was in attendance, Mr M'Keller, are of opinion that there was a ruptured cranial vessel, produced, perhaps, by the fall on the ground during the extreme degree of congestion that must have been produced by the suspension.

" The man was 22 years of age, and of short stature."

The fact, that insensibility and loss of power immediately take place from a complete compression of the trachea, and consequent stoppage of respiration and cerebral circulation has been long known from the murderous acts of the Thugs in India, as well as from some recent crimes of a similar description perpetrated in the streets of London for the purposes of robbery. The constriction of the trachea serves the purpose of the assassin and rober, by not merely rendering the person attacked immediately insensible, but by putting it out of his power either to make any cry whatever for assistance or to offer any resistance. It is the most secret, silent, and rapid means of murder available to the assassin; and hence its recent employment for robbery calls for the severest punishment which the law can inflict. Insensibility requires only a few seconds for its production; and death is the inevitable result, if the constriction be continued in full force for one or two minutes.*

As a curious instance of the ignorance which prevails on this subject among persons who, by education, are supposed to know better, and who profess to instruct the public on questions appertaining to physiology, I subjoin the following letter, which was forwarded to and published in one of the daily papers after the trial and conviction of Drory, and before his confession of the crime, which the writer evidently wished the public to believe might have been an act of suicide.

"Sir,—I think it somewhat doubtful that immediate insensibility is invariably the result of close compression of the trachea. I was once called to a case where a man was found strangled in a bush in the following way :—He was riding home drunk on the top of a cartload of potatoes; he fell asleep ; the horse walked on, approached nearer and nearer to a ditch, and at last one wheel slipped in ; the man fell over, and his neck caught in the angle between two branches, and then the uppermost branch was pressed closely to his neck by the wheel falling upon it and holding it down. In this position it was evident that the man had endeavoured to liberate himself by taking his knife out of his pocket and trying with it to cut away the branch. In that he had failed; but the taking the knife out of the pocket, the opening it, and the three or four cuts made with it must have occupied some time. The man's arm was stiffened in the position in which it must have been when trying to cut the branch. There was no mistake as to the amount of pressure to which the neck was subjected; for the dead body could not be released without the assistance of an axe, and the branch, so thick as to require an axe to divide it, was held down by the whole weight of the cut.—I am, Sir, your most obedient servant.

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* In capital punishment as carried out by the law of Spain (the *garrote*), we have an instance of the rapidity of death by strangulation, when there is a close constriction of the trachea. A collar is placed round the neck, which is capable of being tightened by a screw from behind. In the late execution of General Lopez by the garrote, at Havannah, his head is stated to have dropped forward, and he was lifeless simultaneously with the first turn of the screw.

"*Alford, Lincolnshire, March* 11." "R. U. WEST, Surgeon.

I pass over the absurdity of appealing on auch a question as this to the daily press. The letter itself contains unanswerable evidence of the great difficulty which the writer has experienced in drawing inferences from facts, or even in so recording facts as that just inferences may be drawn from them by others. In the case of Jael Denny there was immediate compression of the trachea in two distinct spots, so complete, as to bruise the tube, and, from the appearances, to obliterate the canal temporarily. In this instance of accidental strangulation, the neck, we are informed, was subjected to pressure, by being caught in an angle between two branches of a tree, and pressed down by another branch from above. The angular space into which the neck fell could not, from this description, have produced immediately if at all, that complete obliteration of the trachea which would be certainly effected by a tightly drawn cord; at any rate, there is nothing to show that such compression would be so instantaneous, as it must have been, by the application of a cord, in Denny's case. The man might, therefore, have breathed slightly for some time, and thus have been able to employ muscular exertions to release himself: the very fact of these exertions having been made, proves that some degree of circulation and respiration must have been going on, and the supposed analogy, therefore, of this case to that of Denny entirely fails. The writer of the letter must either believe that a person on whose trachea a close double ligature has been forcibly applied, might afterwards occupy "some time" in making cuts at the branch of a tree, or his case has no bearing whatever on that of Denny. The application of a ligature to the neck of a dog or cat, so as to include the trachea completely, would have shown him the irrelevancy of the case which he adduces, and the error of the inference to which it would lead. So far as the details of his case are given, it does not appear that it was the trachea, but the neck generally, that was subjected to close compression.

In an experiment performed since this trial, a dog was strangled, not by an amount of pressure "on the neck," but by a ligature directly applied to the lower part of the neck, including the trachea. The animal was instantaneously rendered insensible, unconscious, and incapable of any muscular exertion. The rapid circulation of the unarterialised blood fully accounts for this result.

The evidence in the case of Jael Denny clearly proved that the prisoner had had sufficient time to commit this murder by strangulation. An hour and a-half had elapsed from the time he left his house until he was seen by the witnesses at Brentwood. The distance was about two miles, and might have been walked in half an hour. He set out with the basket of eggs, with the professed intention of going thither; he had, therefore, a full hour at his disposal, but such an act of murder would require only two or three minutes for its complete perpetration; hence, if the time accounted for had not been more than three-quarters of an hour, and the distance from Brentwood even greater, all the medical bearings of the case would have been fully satisfied. There is no death so rapid as that which takes place from strangulation, when it is accomplished by such means as those proved to have been employed in the case of the deceased.

5. *Evidence affecting the accused from stains on his clothes.*—When the prisoner was arrested, some dark stains were observed on the front of a pair of corduroy breeches which he wore, and which he admitted he had worn on the evening of the day that he went to Brentwood. The evidence of the superintendent of police was to the following effect:—

"The blood appeared quite fresh, and the patches were moist or damp. I pointed them out to the prisoner, and asked how they came there. He said it was not blood, but some stuff he had had to give the calves, and if I went with him to his house, he would show me the pot where it was kept. He took me to an outhouse, and, pointing to a pot, said, 'that is the pot where the stuff is.' On taking it down from the shelf, the mouth of the pot was open, and covered with cobwebs. There was a wooden spoon in it, the handle of which did not reach to the edge of the pot, but was quite inside. On showing the prisoner the cobwebs on the mouth of the pot, he said he had not used any of the stuff for a long time."

The stains in front of the small clothes were not so well marked in colour or appearance as some small spots at the back part, in the bend of the ham; these had evidently escaped notice. The stains in front were of a red-brown colour; they had evidently been wiped, so that the blood had been removed from the prominences of the cords, and carried into the deep grooves of the stuff. It was in this situation, and by the aid of a powerful magnifier, that small solid coagula were seen, having the colour and glistening aspect of fresh blood, the fibres of the corduroy around being stiffened. In a few of the small spots at the back part of the clothes, which had not been disturbed, the coagula were well marked. The stained fibres, cut from the grooves of the cords, and examined microscopically, under a power of from 300 to 400 diameters, presented the peculiar form of cotton deeply stained at the tips by a red colouring matter, having the crimson-red appearance of blood; and, in the spots which had not been interfered with, the apex of each cotton fibre was coated with a dark red clot or solid coagulum.

Chemical Tests for Blood.—Chemical tests applied to distilled water, in which a small portion of coagulum and fibre had been allowed to soak, showed that the slight reddish hue acquired by the water was due to blood. Weak ammonia did not alter the red colour, but the application of heat destroyed it. It was a curious fact, and one worthy of note, that diluted muriatic acid, although it did not remove the red-brown colour of a portion of the corded stuff soaked in it, dissolved so large a portion of iron, that it might fairly have been supposed that the red tint was owing to some oxide of iron which had become accidentally fixed upon the stuff. As, however, the red colour was not removed, it was clear that this could not be a satisfactory explanation. A portion of the corduroy, unstained, was then macerated in diluted muriatic acid, and it was found that the acid had dissolved as much oxide of iron from the unstained, as from the stained portion. It was found, indeed, chemically, that the whole of the stuff had been dyed with iron, and that this dye had given the clothes a brownish colour.

A portion of the unstained cord was submitted to destructive distillation, and acetic acid, the usual product of vegetable matter, was evolved. Another portion was similarly tested for nitrogen and sulphur, but neither of these substances was present. Chemical tests so far corroborated microscopical observation, for, under the microscope, the whole texture of the corduroy was resolved into the vegetable fibre of cotton. A stained portion of the corduroy, holding part of a clot considered to be blood, was submitted to destructive distillation, and abundant evidence of the presence of nitrogen (from the production of ammonia) and of sulphur was procured. It was thus demonstrated that the elements of animal matter were present in the stained, but not in the unstained portions of the corduroys.

By maceration of a portion of one of these clots in water, holding dissolved a small quantity of white sugar, a plan suggested by my friend Dr Rees, and by submitting the liquid to a power of from 300 to 500 diameters, under a good achromatic microscope, blood-discs or corpuscles were seen presenting all the usual characters of mammalian blood. Dr Rees examined a portion separately, and came to the same conclusion as myself.

Human and Animal Blood.—As it was considered necessary to determine whether the blood-stains on the corduroy were caused by the blood of an animal, or by that of a human being, another investigation was undertaken, in order, if possible, to form an opinion on this point. Chemical tests utterly fail to establish any distinction; and after many trials of Barruel's process of acting on the blood by sulphuric acid, I can come to no other conclusion than that it furnishes no criterion whatever, and that it would be dangerous to rely upon it in any case. Barruel's process is based on the hypothesis that the blood of every animal contains a peculiar volatile principle intimately combined with it, but capable of being set free by the addition of concentrated sulphuric acid, and then possessing the odour of the perspiration or sweat of the animal from which the blood has been taken. We are told that, *cæteris paribus*, the odour thus evolved, is much more marked in the male than in the female animal, and that in man the colour of the hair appears to exert some influence on the strength of the odour! To produce the result, one measure of blood is mixed with one and a half measure of concentrated sulphuric acid; the mixture is well stirred with a glass rod, and the odour is perceived during the act of stirring. It will be noticed, that the employment of this process presupposes that we have fresh and liquid blood in some measureable quantity, with which we may operate. It is quite inapplicable to blood that has been kept for some time, or to small and scarcely perceptible stains on articles of clothing. Dr Carl Schmidt, of Dorpat, who has lately examined this process, states that, under favourable circumstances, the blood of the goat and the cat may be recognised by the evolution of the peculiar and well-marked odour of these animals; but that in all other cases it gives doubtful, and therefore untrustworthy results. I lately had an opportunity of examining, by this process, the fresh blood of the dog, taken from the vena cava very soon after death. The result was, that it presented no particular odour which could be referred specially to the dog.

Microscopical differences.—An attempt has been made to draw a distinction between animal and human blood, from the different size of the corpuscles. The blood of birds, fish, and reptiles may be distinguished by the shape of the corpuscle; in these three classes of animals, when the corpuscle is in its natural condition, the form of the disc is oval or elliptical; in the human being and in mammalia generally (excepting the camel tribe) it is round. These blood-stains were clearly not caused by the blood of birds or fish, as they presented the well-marked rounded form of the mammalian corpuscle. It was suggested in the defence, that the stains on the clothes might have been produced by the effusion of blood while the prisoner was engaged in killing chickens; but microscopic observation showed that the blood was of the mammalian variety, and therefore this suggestion would not explain the facts.

It is well known, from the researches of microscopists, that the blood of one mammal differs from that of another, in the fact that the corpuscles are larger in some and smaller in others. This, however, is only an average difference; *i. e.*, in the same blood, corpuscles of very different sizes are constantly found; hence, in making use of this criterion, it would be necessary to rely upon the size of the majority of the corpuscles seen in a given area, and under the same power of the microscope. It is a curious fact that the size of these corpuscles in the blood, bears no relation to the size of the animal. Thus, in the horse, ox, ass, cat, mouse, pig, and bat, they are, on the average, of nearly the same size; the difference is so slight as to be practically inappreciable. In these animals they are smaller than in man, and several of the mammalia. The corpuscles in man, the dog, and rabbit, are of nearly the same size. In the blood of the goat they are much smaller than in other mammalia. The size of the corpuscle bears no proportion to the age of the animal; thus in the blood of the human fœtus they are to be found as large as in that of the adult.

The measured diameter of the corpuscles, according to Gulliver, is, in the elephant, 1-2745th of an inch; in the dog 1-3540th; in the hare 1-3607; in the rhinoceros 1-3765th; in the mouse 1-3814th; in the ass 1-4000th; in the pig 1-4230th; in the ox 1-4267th; in the cat 1-4400th; in the horse 1-4600th; in the sheep 1-5300th; in the goat 1-6366th; in the human blood, on the same authority, the diameter of the corpuscle varies from 1-2000th to 1-4000th of an inch; and the average size in both sexes is 1-3200th of an inch.

In microscopical observations, under a power of from 216 to 340 diameters, made on the blood of a dog recently killed, I found that the corpuscles had the usual round form of the human corpuscles. The greater number of these examined in the serum of the blood gave a measurement of 1-4000th of an inch in diameter. A few were larger, but many were smaller; some had a diameter of the 1-6000th of an inch, while the smallest perceptible was the 1-8000th of an inch in diameter. For this experiment the blood had been collected ten minutes after death, from the large venous trunks around the heart, and it was examined within a few hours afterwards.

Two specimens of human blood were examined and compared with this; the corpuscles had an average diameter of 1-3500th of an inch; the maximum size was 1-2000th, and the minimum size 1-5000th of an inch.

Two blood-corpuscles obtained from the stains on the clothes, had a diameter of about 1-3000th of an inch; some were smaller, and others larger. They were compared with those from the blood of a rabbit, a hare, an ox, and a sheep; but even with the aid of an excellent microscope no satisfactory distinction could be drawn between these, and those which were known to be the blood of animals, nor could the latter be distinguished from corpuscles of the human blood. The microscope would here be of good service if all the corpuscles in man or in any one animal had a definite and uniform size, all differing from each other; but this is not the case, and no reliance, as it appears to me, can be placed upon mere average differences, in reference to which, probably, no two microscopical observers would be found to agree. It is sufficient to state, that corpuscles of the same diameter are to be found in the blood of man and of all mammalia (varying in number probably in each case), to show that no satisfactory inference could be drawn in a criminal case respecting the source of the blood which may have caused a stain. For this reason, it was alleged that the stains on the clothes of the prisoner were owing to blood, but that they might have been caused by the blood either of a human being, or of any domestic quadruped. In the defence, it was suggested that the prisoner might have thus accidentally stained his clothing while engaged in bleeding calves.

With reference to the distinction between the blood of animals and human beings, I may here advert to the recent proposition of Dr Schmidt, to dry the corpuscles, and to compare their size in the dried state with an average derived from forty observations on the dried corpuscles of man and the common domestic animals. To procure the standard specimens a glass-plate is just moistened with a very slight film of blood from a wound in a living animal. A film properly taken will vary from the 1-3000th to the 1-12000th of an inch in thickness, and it becomes dry almost instantaneously. A power of 500 diameters is required, and forty corpuscles lying in the same line may be measured from right to left by an accurate screw-micrometer. The results of forty observations, according to Dr Schmidt, gave for the diameter of the dried corpuscles in decimals of a millimeter (=·0393 or about 1-25th of an English inch.)

	Max.		Min.		Average.
In Man	·0086	......	·0070	......	·0077.
Dog	·0078	......	·0065	......	·0070.
Rabbit	·0076	......	·0056	......	·0064.
Rat	·0070	......	·0058	......	·0064.
Pig	·0068	......	·0058	......	·0062.
Mouse	·0067	......	·0055	......	·0061.
Ox	·0062	......	·0054	......	·0058.
Cat	·0062	......	·0052	......	·0056.
Horse	·0064	......	·0052	......	·0057.
Sheep	·0050	......	·0038	......	·0045.

Although the French measurements in decimals of a millimeter are here retained for the sake of accuracy, the figures will allow of a comparison; and they clearly show how minute the differences are, and how unsafe it would be to rely upon this species of evidence as proof. The fact is, no average can be accepted as the representation of truth, but only an approximation to it. I have compared the dried corpuscles of man, the rabbit, the sheep, and the dog, and have been unable to discover that difference which would enable a person to depose with certainty to the species of blood from which the corpuscle was taken. Dr Schmidt's average from forty observations might differ entirely from another average obtained from one hundred or two hundred observations. It will be observed that Dr Schmidt's maximum diameter of the dried human corpuscle is 1-2907th of an inch, and the minimum 1-3571st of an inch: but human corpuscles have been observed much smaller in the recent than his representation gives for the minimum size in the dried state. It is difficult to comprehend how corpuscles should increase in size by the loss of their fluid contents, but without such a supposition his statement does not appear to admit of any explanation. These researches, therefore, do not enable us to solve this difficult question.

Arterial and Venous Blood.—The question whether the stains arose from arterial or venous blood did not arise. There was no sprinkling as from the impulsive force of a wounded artery at a distance. The fixation of the blood in a dyed cotton stuff, as well as exposure to air, would have rendered any distinction from colour simply impossible at the date at which I saw the clothes. Neither chemically nor microscopically can these two kinds of blood be distinguished.

In these cases it is often material to determine whether the staining of the article of clothing has taken place from within or from without. On removing part of the lining of the clothes, it was observed that a portion of one large stain in front had evidently soaked through the stuff, but the spot was much smaller than on the outside. In most of the stains at the back and front the appearance was confined to the outside only; hence it was clear that the clothes must have been stained from the application of blood on the outside.

Living or Dead Blood.—Had the blood which had caused these stains been effused from a dead or living body? From the fact of distinct clots or *coagula* of blood being present in some of the stains, and having a glistening and bright appearance, at the same time adhering firmly to the fibre of the stuff which was stiffened by imbibition of the serous or more fluid part of the blood for some space around, it was inferred that the blood which had caused these stains must have come from the body of a living person or animal, or of one but recently dead, and in whose body coagulation had not taken place. In drawing this inference it is necessary to bear in mind that the already coagulated blood of a dead body may dry in a clot on clothing on which it is accidentally spilled or thrown, and give a slight stain to the parts around. In this instance, however, an examination by the microscope rendered it highly probable that the blood had fallen on the stuff in a liquid form, and had there coagulated. Each pointed fibre of cotton was seen to be invested with a separate coagulum, of a rounded or pyramidal shape, closely adhering to and stiffening the fibre. It is well known that pointed substances presenting a large surface greatly accelerate the coagulation of recently effused blood from the living body. The nature of the clothing was therefore well adapted to produce this rapid coagulation, and to fix the blood on the fibres immediately after its effusion. The answer returned to this inquiry was, that the blood had been effused from a living person or animal, or from one but recently dead.

How long had the Blood-stains been on the Clothing?—By sprinkling a portion of freshly-drawn human blood, as well as that of a rabbit recently killed, on an unstained part of the prisoner's clothing, it was found that after the elapse of twelve hours the blood had produced a dingy red-brown stain in each place, resembling the suspected stains on the clothing, and not to be distinguished by physical appearance or microscopical observation from them or from each other. After the lapse of twelve hours they underwent no further change in colour or appearance, when observed at intervals during many weeks and for several months.

The prisoner was arrested by the superintendent of police about twelve hours after the period at which the deceased had died. The dark-coloured stains in front of the corduroys were observed by him to be then moist or damp. There was reason to believe, from their appearance, that these had been rubbed or wiped with a damp cloth. This furnished some kind of evidence that the stains were then recent. The clothes were delivered to me on the evening of the 14th of October, about thirty-three hours after they had been in the superintendent's possession, and about two days after the time at which the deceased is supposed to have died. They were then quite dry.

It was impossible, therefore, by physical or chemical characters, to fix the date at which these stains had been produced. The only answer that the scientific data justified, as it appeared to me, was, that they might have been produced at any time antecedent to a period of twelve or fifteen hours before I received them ; but whether they had been there two days, two weeks, or two months, it was not possible to determine.

Removal of suspected Stains.—It is important for a medical witness to notice in these investigations, whether any attempt has been made to remove from the clothing, stains which have the appearance of being due to blood. Such an attempt may, in certain cases, afford proof of a guilty consciousness of the cause from which the stain has arisen. This question has been already considered in the preceding remarks. An attempt had been made to remove the two largest stains in front, but, by reason of the thick and corded nature of the clothes, the blood had been, in a great measure, carried into the depths of the grooves, and here it was proved to exist. In concealed parts of the dress, as in the bend of the ham, small spots were found, which there had been no attempt to remove, and these supplied the most satisfactory evidence of the cause which had produced them.

From the fact of no larger vessel being wounded in the deceased, it might have been a question, whence came this blood which had produced the stains on the prisoner's clothing? The deceased had chiefly bled from the nose, mouth, and ears, and, according to one witness, as much as a teacupful had been effused in the spot near which she lay. There was, therefore, enough blood effused during this mortal struggle to produce the marks on the prisoner's dress; at the same time, it must be admitted that it could not be distinctly proved that the stains were caused by the blood of the deceased, or that they had been on the clothing for only a period of forty-eight hours when they first came into my possession. There was nothing incompatible with these assumptions, but, beyond this, the evidence did not go.

Stains from Catechu.—Could these stains have been produced by the substance (catechu), to which the prisoner referred them? After having thus traced, by the aid of chemistry and the microscope, the characters of blood in these stains on the prisoner's clothing, and having had ocular evidence of that infallible test of blood—the existence of corpuscles, it might seem superfluous to entertain the question whether catechu could have produced any, or all of them.

The prisoner himself furnished one of those extraordinary pieces of circumstantial evidence to show the falsehood of his own statement. When the stains on the front of his clothes were pointed out to him, at a time when they were slightly damp, he volunteered to account for their presence by referring them to some medicine which he said he had been in the habit of giving to calves. He pointed out what he stated was the substance, in a stone-jar placed on a shelf in the cow-house. On looking at this in his presence, the mouth of it, (about four inches in diameter,) was so overgrown with cobwebs, and the spoon inside so placed, that it could not have been used, if used at all, for a considerable time. This he admitted when the condition of the mouth of the jar was pointed out to him. But there were other circumstances which proved that this statement was untrue. The stains of variable size found at the back of the clothes, and in the part corresponding to the bend of the ham, were of the same nature as those in front, and could not, from their position, have been produced by the accidental spilling of a coloured liquid in front.

The contents of the jar were, however, submitted to chemical examination. The substance had the appearance, colour, taste, [illegible], and other properties of impure catechu. When pulverised, it formed a light brownish coloured powder, with no red tint in it like the powder of kino. It was soluble in water, and gave, in very minute proportion, the usual indication of its containing tannic acid, when a persalt of iron was added to its aqueous solution. On submitting a portion of it to destructive distillation, it yielded ammonia, showing either that it contained nitrogen, or that it was intermixed with some nitrogenous principle.

When mixed loosely with water, and sprinkled on the corduroys worn by the prisoner, it produced not a red but a dark olive-brown stain, wholly different in appearance from those which had been ascribed to blood. The solid portion of the catechu dried on the stuff, could not be mistaken for a coagulum of blood. It gave with distilled water a solution which was turned of a dark olive-green colour by a persalt of iron. The aqueous solution of a portion of one of the clots from the stains on the corduroys, underwent no change of colour when a persalt of iron was added. The statement, therefore, that catechu had produced the stains, was proved to be untrue by chemical experiments, both negatively and affirmatively, as well as by circumstantial evidence.

In order to complete this investigation, and to establish beyond the possibility of doubt, that no red colouring matter used in medicine or the arts had caused these appearances, I performed a series of experiments by sprinkling an unstained portion of the corduroys with solutions of the various red colouring matters known to chemists. Professor Morton, of the Veterinary College, informed me that the only red colouring matters used as astringents by farriers of the old school of veterinary practice, were Armenian Bole and Dragon's Blood. The former is an insoluble mineral compound, owing its colour chiefly to peroxide of iron; and the absence of this was clearly proved by the chemical examination of the stains. Dragon's blood is a resin insoluble in water, but soluble in spirit; it is used as a styptic. In addition to these two substances, red Sander's wood, kino, and catechu, are also employed.

I am not aware that the changes produced in red colouring matters, by a common article of dress dyed with oxide of iron, has ever before been a subject of remark. I therefore subjoin the results obtained from various experiments:—1. Brazil wood. The stains produced by a decoction of this wood presented when dry a dark puce-brown colour. The stain was of a uniform tint throughout; it presented no appearance resembling a clot or coagulum of blood. Examined by the microscope, the colour was wholly different from that of blood, and could not be mistaken for it. The colour of the stain as well as of the decoction was rendered crimson by weak ammonia; the colour was altered but not removed by diluted muriatic acid. 2. Logwood. The stains were of a dark slate colour; the colour of the decoction was altered by acids, and rendered crimson by weak ammonia. Under the microscope there was no appearance which could be mistaken for the colour of blood. 3. Red Sander's wood. Stain of a dark red colour; altered by acids, and rendered crimson by ammonia; no stiffening of the fibre; no appearance of coagulum; not soluble in water; no change when touched with a persalt of iron. 4. Madder. This produced a dark brown stain on the stuff. Decoction and stain turned crimson by diluted ammonia, and changed to a bright rose-red by a solution of weak muriatic acid. The decoction was darkened by a solution of persulphate of iron. 5. Alkanet. This substance was scarcely so dissolved by water as to give to the liquid a red colour, even when boiled for an hour. It produced a dark brownish stain, bearing no resemblance to blood; there was no stiffening of the fibre, and no coagulum. Ammonia gave a greenish, yellow colour. Muriatic acid destroyed the colour. Persulphate of iron gave an olive-green tint. 6. Archil. The stains produced were of a dark purple-red colour, locking up particles of gritty matter. No stiffening; no appearance of coagulum; colour changed to blue by ammonia, and to bright red by acids. 7. Cochineal. A strong decoction of this beautiful red dye produced a stain almost black.

It bore no resemblance whatever to that of blood. There was no stiffening; no coagulum. The colour of the decoction and stain was changed to crimson by weak ammonia. A persalt of iron darkened the decoction. 8. Dragon's Blood. A red coloured resin not soluble in water, but soluble in alcohol, producing on the stuff a bright orange-red stain, unlike blood; there was no stiffening; no coagulum; and under the microscope no appearance which could be mistaken for blood. Weak ammonia, added to the alcoholic decoction, gave a brownish yellow precipitate; the resin was also precipitated by diluted muriatic acid. 9. *Safflower* A decoction of this substance produced a dark brownish-red coloured stain, which was changed to rich crimson by ammonia, and to an orange-red colour by muriatic acid. 10. *Rose Red.* The red colouring principle of the rose may be taken as the type of nearly all the red colours of fruit and flowers,—thus comprising fruit stains and their effects. A strong infusion of the petal, which had a rich red colour, gave a greenish black stain, not possessing any of the physical or chemical characters of blood. The stain and decoction were changed to a green colour by weak ammonia, and to a bright red by diluted acids. 11. *Cabbage Red.* This colouring principle differs from that of the rose, in producing a rich blue colour, when perfectly neutralised. As the red colour is only well brought out by the aid of an acid, a strong decoction of the leaves acidulated with diluted sulphuric acid, was here employed. It produced a bright red stain, having no resemblance to blood; this was changed to a green by ammonia; and to a bright red by acids. 12. *Red colouring matter of Wine.* The red colour of port wine, concentrated by the distillation of the alcohol, was here employed. The stain produced was of a dark-red colour, bearing no resemblance to blood. It was changed to an olive green by weak ammonia. (Port wine adulterated with logwood or madder would, by this alkali, have its red colour changed to crimson.) 13. *Kino.* This vegetable extract gave a deep red brown decoction with water. It produced on the stuff a dark olive stain, not removed or perceptibly altered by weak ammonia. The alkali does not perceptibly alter the colour of the decoction or infusion; but this is known from blood by the addition of a persalt of iron, which produces an olive-green colour. 14. *Catechu.* This gives a brown decoction with water, producing an olive green stain; the chemical properties are the same as those of kino. It may be observed of these two colouring matters, that they are extracts containing that variety of tannic acid, which yields a green colour with a persalt of iron. They strikingly differ from blood,—in the fact that the colour of their solutions in water, is wholly different in tint, is unchanged at a boiling temperature, and is turned to a dark olive-green tint by a neutral solution of a persalt of iron.

From these results, it will be perceived that there is no red colour used in the arts or medicine, which would produce on the clothes, stains possessing the physical and chemical characters of those found upon them. It was further proved by affirmative experiments, that the blood of the rabbit and of a human being would produce stains precisely similar to those found.

It is one of the curious scientific facts in the case, that those substances, which on common articles of clothing would produce red stains that might be mistaken for blood in a hasty examination, produced only black or olive-green discolorations on the corduroys worn by the prisoner. The chemical reason for this will be at once obvious. These vegetable colours contain tannic acid; the corduroys were impregnated with oxide of iron as a dye: hence the change. In the blood there is no tannic acid or other substance to combine with oxide of iron; and thus the red colour was preserved, with the exception of that slight brown tint which hæmatosine acquires when dry and exposed to air.

The stiffening of the cotton fibre around the stained spot has been adverted to as a character of blood. This arises from the albumen of the serum. It might be found in gummy decoctions, but it ought not to be absent when the stain is really due to blood, and the stained substance has not been interfered with.

The presence of a coagulum in the stain has also been adverted to as a character of blood. Care must be taken that coloured particles of organic matter in the midst of a stain are not pronounced to be coagula. In the course of these researches, I found that stains caused by Kino, in which the undissolved powder had not been separated by filtration, presented, in some instances, under the microscope some of the physical characters of a blood-stain. By artificial light, the red tint given to the cotton fibres presented so strongly the colour of blood, that it might have easily been mistaken for it. This is a new fact to myself, and I point it out as a caution to others. It demonstrates that, unless the corpuscles of blood be found, the microscope may mislead one who is not much accustomed to these researches, and who may, therefore, be inclined to rely upon colour and physical characters alone. On the other hand, when the corpuscles are not found, a solution in cold water of the substance giving the appearance of a coagulum, will enable the chemist to come to a conclusion respecting the nature of the stain. If it be due to kino, the colour will not be destroyed by heat,—there will be no appearance of a coagulum on boiling,—and a persalt of iron will strike the dark olive-green tint, characteristic of the presence of tannic acid. These characters it is of the greater importance to bring out, since weak ammonia does not produce any change of colour in a solution of kino. I have not found any other vegetable substance to give the appearance of a coagulum possessing the colour of blood.

In concluding the remarks on this case, I would observe, that it is one well calculated to impress our minds with the conviction, that an atrocious crime perpetrated in secrecy may be detected by a proper investigation, and brought home to a criminal with a certainty as great as if he had been seen to commit it. It is impossible, either in a moral or scientific point of view, so to dress up falsehood as to make it pass for truth.

Art. VI.—*Researches upon the Treatment of Acute Articular Rheumatism by the local application of Anæsthetic Agents.* By M. Dr Aran, Physician to the Central Board of Hospitals (Bulletin de l'Academie de Medecine). Meeting of 19th December 1850. 23d December 1850. (Condensed from various articles in the Archives Generales, Quatrieme Serie, January 1851. February 1851. Tome xxv. Paris, 1851).

M. **Aran** has been led to adopt this new mode of applying anæsthetic agents, by the success which he obtained from th
local application of these agents, in the treatment of *hydrarthrosi*
mono-articular, sub-acute, and chronic rheumatism.

The treatment employed by M. Aran consists in applyi
over all the diseased articulations a wet compress, on which h
been poured some drops of an anæsthetic agent, and princip
of the chlorated chlorhydric ether. This compress is kept f
round the joint by means of a dry compress, or by a coverir
oiled silk secured by a bandage. In the first trials M.
left the compress in its position for twenty-four hours wi
renewing it; but he found that it was advantageous, wh
pain had not entirely left the joint, to resume the applica
the evening, at the period when rheumatic pains usually
an exacerbation. So long as there is pain in a joint, pai
pressure, or pain upon moving the joint, M. Aran contir
local anæsthetic applications. If a new articulation is
by pain, this is treated by the same applications; and th
thus pursued from one joint to another, until the rh
has entirely disappeared.

Since the month of April 1850, M. Aran subject
treatment, both in his own service at the Hospital Bo

and in that of M. Bricheteau, at the Hospital Necker, eighteen patients, of whom five were labouring under very acute rheumatism, eleven under acute rheumatism, and two under moderately acute rheumatism. This division rests upon the degree of intensity of the fever and of the pains, and that of the general distribution of the disease. The results obtained may be distinguished into primary and consecutive.

The primary results are the following. Complete tranquillity immediately after each application, varying in duration from one hour to six or eight hours, according to the acuteness of the disease, and more especially according to the acuteness of the febrile condition. It is requisite, says M. Aran, to have witnessed the change induced by these applications, in order to comprehend the amount of their importance. Patients fixed in a state of immobility by their pains are enabled immediately to execute movements and turn themselves in bed. Under their influence, sleep, which was previously disturbed and gone, returns, and a considerable number of patients sleep immediately after the applications. Further, at the same time that the pain is diminished and disappears, the swelling, the redness, the intra-articular effusion disappear also the ensuing day; in other instances, nevertheless, only a diminution in these morbid phenomena takes place. M. Aran thinks also that he has observed that the articulations, which have been detumefied by means of local applications, have little tendency to be again attacked by the disease. *Lastly*, the febrile movement generally declines, at least from a general condition, or from an inflammatory complication.

The definitive results are the following. Medium recovery; for attacks of very acute articular rheumatism on the tenth day of treatment, or on the eighteenth day of the disease; for attacks of acute rheumatism on the seventh day of treatment, or on the fifteenth day of the disease; for attacks of moderate acuteness on the sixth day of treatment, or on the sixteenth day of the disease.

Among eighteen cases of rheumatism, ten were complicated at the instant of the commencement of the treatment, with endocarditis, old or recent, namely, the five cases of very acute rheumatism, and five cases of acute rheumatism. Of these ten patients, five presented complications towards the chest; among the five patients, two had already an organic affection of the heart, and four had one or several attacks of rheumatism.

The alterations observed were, in two instances, a twofold pleuritic effusion; in one case, a pleuro-pneumonia, and in one case, a double pleuritic effusion with pericarditis. All these five cases got well under the employment of blood letting and the application of blisters.

From these facts, M. Aran deduces this conclusion, that, in the instances in which the febrile movement is very acute, there is ground to employ, along with the local applications, general blood-letting. In this manner he treated, with one or two blood-lettings in the course of twenty-four hours, and with anæsthetic applications upon the joints, five patients, one of whom was labouring under very acute rheumatism, and four under acute rheumatism, producing a complete cure in nine days. M. Aran, in short, allows that this mode of treatment by local application, does not exclude other approved therapeutic methods.

At a subsequent meeting, on the 23d December, M. Aran communicated to the Academy, upon local anæsthetic treatment, a note containing several important facts.

M. Aran refers to the experiments of MM. Serres, Flourens, and Longet, in proof of the proposition, that ansæthetic agents exert upon the nervous chords a local influence in which they extinguish sensibility. But this was scarcely requisite; for it is well known, for a very long time, at least since the days of Robert Whytt and Francis Home, that anæsthetics do act locally, whether upon the nerves or not. He refers also to experiments performed by Dr Simpson and other physicians, both in England and France, in proof of the inference that by means of local anæsthetic applications, muscular rheumatic pains and neuralgic pains are alleviated. The success obtained encouraged M. Aran to continue his researches, and he has obtained results, which, though not complete, may probably serve as a foundation to new applications and further inquiries.

It has been seen that M. Aran did not use, and does not recommend the use of chloroform. He found, in short, that there was a peculiar difficulty in making use of this agent for local anæsthetic application. Chloroform produces a sensation of scorching and intolerable burning so severe, that patients obtain relief by very acute sufferings, and sometimes even after sustaining a burn in the first or in the second degree. Anæsthetic properties in different degrees are found to reside in a very great number of hydrocarbonated and chloro-hydrocarbonated substances. After having tried these different substances, his attention was attracted by a particular product discovered by M. Regnault, and to which this learned teacher has given the name of *chlorated* chlorhydric ether. On this subject M. Aran received from M. Mialhe a note, of which the following is the most important portion.

"M. Aran having requested me to place at his disposal the different volatile agents in which anæsthetic properties have been recognized, with the view of his studying with greater care than had been hitherto done, the local and sedative action of these agents, I sent him at two different periods liquids obtained by the reaction of chlorine upon bicarbonated hydrogen, which had been supplied to me by two of the best manufacturing chemists in Paris, under the name of *Dutch Liquid.* The first of these liquids gave to M. Aran very satisfactory clinical results, which he has lost no time in making known. With the second it has been different. I felt it necessary to inquire into the causes of this difference in results, and I found that the last of these liquids alone possessed the character of the Dutch liquid; while the former presented more relations with the chloride of liquid carbon than with the Dutch liquid properly so called, presented a higher degree of density and a boiling point than this liquor, and further was not inflammable. Pursuing my comparative researches, I obtained certain proof that this liquor was not chloride of carbon, but Dutch liquid, which had lost a certain amount of hydrogen, and which had acquired an equivalent proportion of chlorine, that is to say the *chlorated* Dutch liquid. The formation of this last body is easily explained. An excess of chlorine entirely decomposes the bicarbonated hydrogen gas, or olefiant gas, by eliminating successively its equivalents of hydrogen, in the state of chlorhydric acid, by substituting itself in its place, and given rise to a series of compounds, more and more rich in chlorine, and the last term of which is the sesquichloride of carbon of Faraday.

"It is therefore certain that the favourable therapeutic results recently published by M. Aran ought to be ascribed to the chlorated Dutch liquid, and not to the Dutch liquid properly so called. But the price of the chlorated Dutch liquid being too costly to allow this substance to be advantageously introduced into the practice of medicine, I have thought of substituting in its place an analogous ethereal compound, arising from the action of chlorine upon chlorhydric ether. By causing an excess of chlorine to react upon chlorhydric ether, we obtain a preparation, presenting all the external characters of the ethereal liquid, resulting from the action of an excess of chlorine upon bicarbonated hydrogen. This new compound, employed experimentally upon patients by M. Aran, has completely confirmed the supposition now made, and has been found endowed with the same therapeutic virtues as the chlorated Dutch liquid. This compound is colourless, very fluid, having an ethereal aromatic odour analogous to that of chloroform, or rather to that of the Dutch liquid; a taste at once saccharine and spicy, or like pepper. It exerts no action upon turnsol paper; it is scarcely soluble in water, but is perfectly dissolved in alcohol, in sulphuric ether, and in the most of the fixed and volatile oils. It is not inflammable,—a circumstance which distinguishes it from the Dutch liquid and the officinal ethers, and which causes it on the contrary to approach to chloroform in nature. It presents a variable density and a boiling point equally variable, oscillating between 110° and 130° of single substance, but by the union of several others, differing in the Centigrade thermometer=230°and 266of Fahrenheit. These characters evidently show that this body is not constituted by one density and in elastic tension. As these different chlorated chlorhydric ethers all possess the same anæsthetic properties, and as it is impossible to separate them accurately from each other, I propose to designate the liquid which they form by the general name of chlorated chlorhydric ether."

In his further trials, M. Aran found that this agent was the most eligible for the purposes which he had in view, and after many trials he was further satisfied that the liquid form was that which was most suitable, whenever it was the object to produce a speedy effect. He continued for several months upon patients the study of disorders in which its application was admissible and advantageous; and while he finds the number of these disorders to be considerable, he thinks, without saying that his researches are final, that his observations and experiments entitle him, in the meantime, to draw the following conclusions.

1. Local anæsthetic properties are found to reside in all those agents in which we have hitherto recognized general anæsthetic properties, and in those, even solid, which present with them analogies in composition; principally in the series of chlorohydrocarbonated bodies.

2. The local anæsthetic properties are not in direct proportion to the general anæsthetic properties, but rather in the degree of fixation of the substance. The more volatile it is the less marked is its anæsthetic local action. This explains the relative inferiority of the local anæsthetic properties of sulphuric ether, in comparison with other anæsthetic substances.

3. A great number of anæsthetic agent possess properties irritating to the skin. The first rank in this respect is occupied by chloroform. When applied locally, it may induce an actual burn in the first or in the second degree.

4. The anæsthetic agent most easily managed, most certain in its action, and at the same time least irritating, is the *chlorated hydrochloric ether*. The sesquichloride of carbon may also be advantageously employed with the same object; but while the action of hydrochloric ether is complete after the lapse

of some minutes, two hours at least are required in order to produce insensibility by means of the sesquichloride.

5. In order to obtain sufficient anaesthetic effects it is not necessary to employ these two anaesthetic agents in a very large amount. Fifteen, twenty, twenty-five, thirty drops at most of chlorated hydrochloric ether, poured upon the painful part, or upon a wet cloth, which is to be applied immediately over it, and which is to be maintained in contact with the part by means of oil-cloth, and a turn of a bandage, very rapidly abates pain, and produces *anaesthesia* in some few minutes. The practitioner may also employ the chlorated hydrochloric ether in the form of pomatum or ointment (four grammes to ten grammes of lard) or the sesquichloride of carbon in the same form, four grammes to ten of lard. Both of these ointments are employed by means of friction, or they are simply anointed over the diseased parts.

6. In a physiological point of view;—anaesthetic agents in general, and particularly chlorated hydrochloric ether, are followed, after an interval which varies from two minutes and a half to ten minutes, by the complete cessation of pain in the parts suffering, and after a space of time, which varies from five to fifteen minutes, by a degree of cutaneous sensibility, which is very easily measured by the point of a needle. Sometimes the puncture is not felt; sometimes it is felt only in a feeble manner; but in order to make the comparison in a satisfactory mode, it is requisite to take parts of the skin a little removed from the spot on which the hydrochloric ether has been applied.

7. Insensibility induced by anaesthetic application is by no means confined to the point upon which the application is made; the deep-seated parts themselves have their sensibility diminished. Thus by applying anaesthetics over the skin we abate the pains of muscular organs, of nerves, of articular cavities, of organs deeply-seated in the cavities of the chest and of the abdomen. Further, anaesthesia does not remain always confined to the point at which the anaesthetic agent has been applied. It proceeds beyond this point to a variable extent, but which is rarely less than two square inches.

8. The duration of insensibility varies according to the nature of the anaesthetic agent employed, the quantity of the substance used, and the time during which it is in contact with the skin. It is about from half an hour to one hour, for states of anaesthesia induced in the physiological condition. It is a great deal longer in cases in which the application has been made with the intention of producing insensibility to pain or *analgia.**

9. In a medical point of view the number of cases in which local anaesthetic agents may be applied is truly immense. The numerous clinical researches made by M.

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* M. Aran uses the term *Analgesia*; but this term is not formed according to any correct analogy,

Aran upon this subject for several months have led him to form the following conclusion, which he regards as of great practical importance.

Whenever there exists acute pain in any point of the animal economy, whether this pain constitutes by itself alone the disease, or forms only an integral and principal part of the disease, we may without inconvenience deliver patients from such pain, for a space of time more or less lengthened, by means of one or several local anaesthetical applications.

The proposition now stated gives a summary view of all the applications which may be made of the local anæsthetic method. M. Aran allows that occasions have not been afforded to make trial of this method in all the circumstances in which it appears naturally indicated; but he specifies some of the therapeutic results obtained.

The employment of anæsthetic applications in articular rheumatic pains and in neuralgic pains, M. Aran presumes to be sufficiently well known. These applications cure neuralgic pains, he allows, in a definitive manner, only when they are recent in origin.

In subacute and chronic articular rheumatism, by themselves alone, they cure in some minutes the patients of their pains. In instances of subacute and chronic *arthritis*, they also abate these pains; but, above all, they are serviceable in rendering the patients capable of allowing the immediate application of certain surgical remedies, for instance compression. In one case M. Aran succeeded in restoring, after previous application, and without pain to the patient, a limb which was retracted in consequence of chronic *arthritis* of the knee-joint. By the preliminary application of anæsthetic agents, previous to the use of certain surgical measures, he expresses the opinion that practitioners may render great services to patients.

It is principally, however, in acute articular rheumatism that local anæsthetic treatment shows its efficacy. The relief afforded by these means restores instantaneously to patients, sleep, and the use of their limbs. The course of the disease is abridged; and convalescence speedily follows. It has been already stated that this local treatment does not exclude the use of suitable internal and general remedies, especially blood-letting, which tend to promote the resolution of the disease.

Lastly, M. Aran states that he has treated in the same manner, and with as great a degree of success, the visceral pains of lead colic, nervous colics, uterine and nephritic pains, the pains even of puerperal *peritonitis*, the pricking pain of pleurisy, the pain of pericardial inflammation; and in all cases, without exception, he obtained, if not the complete and final disappearance of the phenomenal pain, at least an unexpected degree of amendment and alleviation.

The statement of the facts now mentioned, and the representations of M. Aran upon the local anæsthetic effects of the chlorated hydro-chloric ether, induced M. Flourens to make various experiments upon animals in order to ascertain the physiological action of this agent. A notice of these experiments was communicated to the Academy on the 13th January 1851, that is, three weeks after the statements of M. Aran were given. In connection with these observations of M. Aran; therefore, it is not unseasonable to advert to the observations of M. Flourens upon the effects of the chlorated chlorhydric ether upon animals.

As to the general anæsthetic effect, all the dogs subjected to the inhalation of this ether were affected by general anæsthesia in a very few instants, some at the end of three or four minutes, and others at the end of four or five minutes. The sciatic nerve exposed in some of these dogs had lost all its sensibility; but it retained its moving power. None of the animals died under this experiment.

Injection into the right crural artery of several dogs, and by impelling on the side of the heart from two grammes to two grammes and a half of chlorated chlorhydric ether, produced the following effects; at the instant of injection, pains and cries from the animal; when the injection was completed, sudden palsy of the posterior part of the body, with tetanic rigidity of the two legs. Lastly the sciatic nerve exposed, still retained its sensibility, but had lost its moving power.

It may be observed, that if this experiment is correctly reported, the result is exactly as to the nervous system of the animal the converse of the last. It appears, in short, that the chlorated chlorhydric ether when inhaled diminishes, impairs, and abolishes sensibility, but little or not at all affects the moving power; whereas, conversely, when injected into an artery, it diminishes, impairs, and abolishes the moving power, but little affects or leaves entire the sensibility at least of the motive nerves.

From these experiments M. Flourens draws the conclusion, that chlorated chlorhydric ether, whether it is respired or injected into the arteries, has the same action as chloroform.

Chloroform, when injected into the arteries produces immediately palsy of the muscles, with tetanic rigidity. The same effects are also produced by injection of the essences of turpentine, of mint, of rosemary, of fennel, and similar substances. On the contrary, the ordinary ethers, alcohol, sulphuric acid, ammonia, camphor, produce palsy of the muscles with relaxation. Thus, of different substances injected into the arteries, one class separate in the nerve, sensibility from the motific power; and the other separate in the muscle, the force which causes rigidity, which causes tension, from the force which induces relaxation.

The same experiments appear further to establish a line of separation between muscular action and nervous power or energy; for, on the one hand, tetanic rigidity appears at the very time when the motific power of the nerve is lost, and on the other hand, muscular relaxation is established at the time when the moving power of the nerve subsists. There is therefore a visible independence between the action of the nerve and the action of the muscle. These experiments M. Flourens, justly remarks in concluding, furnish a new means of physiological analysis, and probably that which is the most delicate and the most efficient that physiologists have yet been able to employ.

ART. VII.—*On Fatty Diseases of the Heart.* By RICHARD QUAIN, M.D., &c. (Medico-Chirurg. Trans., Vol. xxxiii. No. ix. p. 121.) London, 1850.

DR QUAIN arranges the facts which he has collected on this subject in the following order:—

I. The varieties of Fatty Diseases of the Heart, and their respective characters.
II. An account of preceding observations on these Diseases.
III. A consideration of the circumstances under which they occur—their causes.
IV. Their effects on the Structure and Functions of the Heart.
V. Their Symptoms and Diagnosis.
VI. The Indications for their Treatment.

Appended are found Tables containing the abridged histories of eighty-three cases, in which fatty disease of the heart existed. Of these cases twenty-five are derived from original sources,* and the others from different publications, with additions, in several, derived from inquiries made of the authors. Drawings illustrative of the morbid conditions described are also annexed.

I. THE VARIETIES OF FATTY DISEASES OF THE HEART, AND THEIR RESPECTIVE CHARACTERS.

There are two forms under which fat presents itself as a disease of the heart. In one of these forms the fat, composed of large cells containing oil, identical with the fat found in other parts of the body, grows upon and extends over the surface of the heart; it then encroaches on, and insinuates itself between, the muscular fibres, in some cases to such an extent as to completely conceal them when the examination is made with the unassisted eye. In the other form of disease, a fatty matter, composed of granules and small oil-globules, occupies and fills the sheath of what was previously muscular fibre. In the one form of disease fatty tissue grows on and outside the fibre, in the other the muscular fibre itself degenerates into molecular fatty matter. A distinction thus marked in the anatomical characters of these two forms of disease, and in their mode of origin, requires to be clearly expressed in the designations given them. It will, therefore, be well to apply the term fatty growth to all cases in which fat tissue constitutes the morbid condition, even though it may encroach so far on the muscular fibres as to cause their apparent transformation into fat; whilst the term fatty degeneration will be limited, as in strictness it should be, to those cases in which the muscular fibre degenerates into molecular fatty matter. The necessity for this distinction will be evident as we proceed.

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* A few of the cases thus obtained have appeared in the journals from time to time since they were placed in these tables. Mr Paget's and Dr Ormerod's valuable cases have been published so recently, and in so distinct a form, that Dr Quain thought it not necessary to introduce any of them here.

a. Fatty growth on the heart.—The fat found external to the heart's fibres consists, as already stated, of oily matter contained in cells, and is nearly identical with the fat in other parts of the body. These cells are found oval, spherical, or polygonal, the forms being dependent on accidental circumstances, and when full-sized they measure about $\frac{1}{300}$th of an inch in diameter. The yellow tissue composed of these cells, blood-vessels, &c., is first seen, and is always most abundant in the groove between the auricles and ventricles. The distribution of fat here bears a relation, as it does in other parts, to that of the blood-vessels. Thus it first appears in the course of the primary branches of the coronary vessels, then in that of the secondary branches, that is, in the groove over the septum, which marks the boundaries between the ventricles, and lastly follows the distribution of the smaller lateral branches. These branches are more superficial over the right ventricle than over the left, hence the right is always found first and most abundantly covered with fat. A fringe of this substance is also found at the apex of the heart, and frequently around the margins of the auricles. The course of the fat has been described by Senac,* Haller,† Laennec,‡ M. Bizot,§ M. Rokitansky,‖ Mr Paget,¶ and others. The fat may so completely envelope the heart, that none of the muscular tissue can be seen on its external surface. A mass of fat of this nature may of itself be sufficient to oppress and embarrass the heart's action; but fat rarely exists in such abundance on the surface of the organ without insinuating itself between and encroaching on the heart's fibres. In this way the muscular parietes of the organ become thinner and thinner, until the columnæ carneæ appear to arise from a mass of fat, as they are described to do by both Laennec and M. Bizot. This state constitutes what some writers have regarded as fatty degeneration, what M. Rokitansky has called "fatty metamorphosis," but which is, in reality, nothing more in many cases than an hypertrophy of fat. In hearts or parts of hearts less affected, that is where the fat is not very abundant, simple striæ of yellow tissue will be observed lying amongst the fibres. This appearance is common in the auricles.

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* Traité de la Structure du Cœur, 1749, vol. i., p. 187.

† Elementa Physiologiæ, 1757, lib. iv., sec. 3, p. 324.

‡ On Diseases of the Chest. Translated by Dr Forbes. 4th edition; London, 1834, p. 583.

§ Mémoires de la Société Méd. d'Observation; Paris, 1837, vol. i., p 351.

‖ Handbuch, der Pathologischen Anatomie. Von Carl Rokitansky. Wien, 1846, band iii., p. 459.

¶ Medical Gazette, vol. ii., 1847, p. 229.

When portions of the heart suffering from fatty growth in a high degree are examined with the microscope, it will be found, that where the growth is most advanced, that is always the external surface, few muscular fibres can be seen, and the wide intervals between them are occupied by fat cells. Proceeding inwards, we find the fibres become more evident, the fat cells fewer, and finally we have the fibres beneath the endocardium with a few cells lying here and there amongst them. It is deserving of attention, that the fibres, even though they are overwhelmed with fat, may still retain their organization; but in all cases, the course and direction of the fibres are more or less modified and distorted. Thus, then, there is of necessity neither a degeneration nor metamorphosis of the muscle, but a growth or hypertrophy of fat upon it, though in many cases some of the fibres will be found degenerated. The fibres still existing, though concealed, explain the persistence of the heart's action in those cases in which the muscular walls appear to a greater or less extent replaced by fat.

A curious appearance, sometimes presented by the fatty tissue forming little masses within the heart, should be mentioned here. These little masses appear beneath the endocardium, and are raised above the surface of the ventricle. They are in size from that of a pin-head to that of a pea, and they have been accurately described by Lancisi,* M. Bizot,† and Dr Peacock. I have seen these little fatty tumours in two cases. There was a considerable but not an excessive amount of fat on the surface of the heart in both these cases.

b. Fatty degeneration of the heart's texture.—The general characters of this morbid change will be more readily comprehended the alterations which the fibres undergo, as shown by the scope, have been described. When a portion of the heart diseased is examined, the first thing which meets the eye is it of the transverse striæ which mark the fibres of all the tary, and less distinctly those of the heart amongst the in- tary muscles. When a portion of the object in which the se has but commenced is examined, it will be observed, that dition to the faintness with which the cross markings are a number of dark small dots stud the fibres in many points, ntly situated within the fibre. A few such dottings do not ar inconsistent with a tolerable healthy state of fibre, and they equently found in hearts which have well performed their func- . As the observer, however, approaches the seat of more led disease, he finds that the cross markings have in many ts disappeared, and that the black dots have increased in size number; some of the dots are now found to be transparent e centre, and to assume some order in their arrangement. etimes the centre of a fibre is occupied by a long row of ue points, forming a continuous line in the course of the ent, sometimes two or three lines lie side by side, the dark being broken by transparent apertures or circles. Less ently the lines lie transversely to the fibre. These appear- s are well represented in fig. 3, Plate III.* Thus may be d, as Mr Paget has described, the change from the particle olecular fat, the black dot in the fibre to the large oil globule its transparent centre. These granules and globules become numerous, they finally occupy the whole fibre, and cause it esent the appearance well shown in fig. 2, Plate III., or in a more marked manner by a higher magnifying power, and in a advanced form of disease, in fig. 1, Plate IV. The fibres, as shown in fig. 2, Plate III., present much the appearance nd cannot be compared to a more like object, than the tubules e kidney, when the cells of that organ are the seat of fatty neration. This fat, be it remembered, is not in the ordinary lls, such as are found on the surface of the heart or amongst ibres. The fat globules placed within the fibre are much ler, appear to have a mere albuminous envelope, and are ex- ely like the oil globules of milk. They often escape from roken fibres, float as free oil globules, or lodge between the , and give the appearance, as M. Rokitansky believed, of partially placed external to them. Mr Paget and Dr rod have fully confirmed the observation, that they are situ- within the fibre itself. It will be further observed, that the sed fibres are so friable, that they break up readily into small ited fragments or short masses. In making a section, it is times possible to obtain a small diseased point occupying

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* De Motu Cordis Romæ, 1728, p. 55.

† Mémoires de la Société, &c., p. 357.

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hese references apply to the Plates in the Medico-Chirurgical Transactions.

two or three fibres surrounded by tolerably healthy texture. In such a specimen the whole series of changes may be followed until we arrive at the centre, where the disease is fully developed, and where we find the fibre broken up, and the field almost entirely occupied by oil globules. The size of the globules of fatty matter rarely exceeds that of the blood corpuscles, that of the black dots may be, in the first instance, not more than one tenth of this measurement. That these particles and globules are composed of fat is demonstrated by their highly refractive properties, and by the action of ether upon them, when they have escaped from the sarcolemma. All parts of the heart's muscular fibre are liable to this change, but not equally so. Thus it is most frequently found in the left ventricle, next in the right ventricle, then in the right auricle, and least frequently in the left auricle. It is generally more evident in the columnæ carneæ, and inner layers of the muscular fibres than elsewhere. As already stated, the heart may be affected throughout, or the change may be limited to a portion of either side. Of twenty-two cases in the series, in which the seat of the disease is expressed, in ten both ventricles were affected, in eight cases the left, and in four the right.

General appearance of a heart affected by fatty degeneration.

When we recollect, that the firm, highly organized muscular tissue, is nearly replaced by a fluid or semi-fluid substance possessing the characters of oil or soft fat, we can readily form a correct idea of the nature of the appearances which the examination of the heart itself will present. Thus we invariably find a change in the colour and consistence of the organ. The colour is pale, sometimes being as pale as the palest dead leaf; more generally it is of a yellowish brown, a buff, or muddy pink colour. This appearance is not uniform. It is generally in spots or patches, and though the whole heart may be pale, the spots being still more so, when seen beneath the endocardium, give the tissue a mottled or spotted appearance. The spots are of infinite variety of size and form, such as lines, crescents, circles, &c. They may also be seen beneath the pericardium, and in the substance of the heart. As the disease advances, the spots run together and give portions of the heart an uniform buff coloured appearance. This appearance may be confined to a portion of the heart, the rest presenting the ordinary healthy characters. The consistence varies also. It may amount to a mere softness or flabbiness, or be such as to permit the heart to be torn like "wet brown paper." The organ then feels like a piece of wet chamois leather, or "a wet glove." In some cases the heart thus affected retains, in appearance, much of its ordinary solidity, but the tissue breaks down by pressure exactly as would a lung consolidated by pneumonia. This state more frequently occurs in hypertrophied earts. These differences in the amount of change in the colour nd consistence appear to be connected with the causes on which he diseased condition depends, when this can be referred to a ocal modification of nutrition—such as obstruction of the coro-ary arteries—a small portion only of the heart may be affected ut in the highest degree. It is then that the fatty degeneration ccurs to a limited extent, but in its most marked form. Such is he condition which appears to have been noticed by Laennec paricularly. When, however, the origin of the disease can be reerred to a more extended lesion of the nutritive function, a greater extent of the heart may become diseased, but in a less degree. In the one case we have complete and extreme fatty degeneration, in the other merely the pale soft flabby heart, which has been described as granular degeneration of the organ. Thus, then, we have two forms of degeneration of the muscular fibre fundamentally the same, and differing merely in extent and degree. Between these two forms I make no essential distinctions, save those of extent and degree. In addition to the changes of colour and consistence, we also may remark other appearances in connection with this morbid process. The fibrous character of the heart's structure even to the naked eye disappears, and in some cases the tissue resembles that of a fatty or boiled liver. In other cases, the cut or torn surface has a granular aspect not unlike that of the surface of the lung in an early state of grey hepatizaion. These different appearances may be in some measure due o the greater or less fluidity of the oily matter present, as well as o the extent and degree to which the disease has advanced. In ne case which came under my notice during frosty weather, the orn surface of the fibres presented the granular glistening chaacter of the fractured ends of a piece of steel. Varieties also in ppearance are caused by the presence of a greater or less quanity of blood or its colouring matter in the heart's texture or in ts cavities, by which the lining membrane may, in the latter case, e dyed of a deep purple colour. These lesser modifications do ot, however, interfere with the leading characters of the disease, which are paleness, softness, peculiar mottling, and friability of the heart's texture, changes which, with the altered character of the fibres as shown by the microscope, will leave no doubt, when present, of the existence of this morbid change. These alterations must, however, in some cases be looked for with care. In those cases in which the disease is much advanced, the quantity of fat present is so considerable, that the greasy appearance is at once evident to the naked eye. The scalpel used in cutting the tissue is greased, and so is blotting paper when applied to the cut surface. In other cases, however, in which the disease is far more extended, perhaps, but less advanced, these ordinary characters of the presence of fat may be so obscure, that they may escape notice in a superficial examination, and it is this fact which has caused the disease to be overlooked in the absence of those more efficient means which are now used for ascertaining the presence of these minute changes in structure. When possible, these means (I allude to the microscope) should always be applied to the investigation of this disease. The specimens should be examined in as nearly a recent state as they can be obtained; and different portions of the heart, if soft and easily broken, or paler than natural, should be examined without reference to the existence or non-existence of spots or mottling. The pale soft flabby hearts, which have been described by many writers, are without doubt examples of this degeneration.* In all their characters, they resemble those in which the disease has been traced with the microscope. With the view of comparing the two classes of cases, I have placed all the latter cases in a separate series. Thus we shall have of fatty degeneration, fully recognised, thirty-three cases. Of flabby soft hearts, thirty-five cases; and of fat heart, fifteen cases; in all, eighty-three cases.

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* The nature of this softened state of the heart has been a source of considerable difference of opinion amongst pathologists. Kreysig, and M. Bouillaud (Malad. du Cœur, tom. ii., p. 292) connect this condition with carditis, and describe a red, a pale, and a yellow softening. Hope (Diseases of the Heart, 4th edit., p. 310) follows M. Bouillaud, but thinks the pale and yellow varieties may sometimes occur in the absence of inflammation. Laennec (Diseases of the Chest, &c., p. 565) describes this state of heart well. He says, the pale softening resembles in all respects the fatty degeneration which he also describes, save in not greasing paper. He denies the connection between this softening (in the absence of endo-or pericarditis) and inflammation, and attributes its origin to cachexia of the system. Akenside (Philosophical Transactions, vol. iii., p. 353) gives the history of a remarkable case, which he calls "gelatiniform softening." It occurred, as fatty degeneration frequently does, in a heart which had been the seat of former pericarditis. The subject was aged 14 years. Yet M. Chomel (Dict de Méd.. 2d edit., art. Ramolissement du Cœur), rendering the matter more confused, states this softening, in a youth of 14, to be the same thing as that described by M. Blaud (Bibliothêque Med., tom. lxviii., p. 364) as "senile softening" of the heart. One cannot read the details of the cases given by these different writers without feeling convinced that, in the majority of cases, they are describing the same thing, and that this is fatty degeneration. Every sound pathologist knows that true carditis, by itself, is excessively rare (Meckel. Hist. de l'Academie, vol. xii., p. 33; and Baillie, Morbid Anatomy, 3d edit., p. 19), if it ever occurs. In low forms of fever the heart is softened, the cohesive power of its molecular elements is diminished; the markings on its fibres are faint; and the tissue is easily crushed. It seems to me, highly probable, that all these varieties of softening, except that just mentioned, are more or less intimately connected with this fatty change in the fibre. M. Blaud, indeed, offers no other objection to such an idea in reference to his cases, than that fat is not seen overloading the fibres. Even M. Cruveilhier (Anat. Pathol., tom. i., liv. iii.) is astray here. In describing "apoplexy of the heart," he notices the fragility and discoloration of the fibres; and two of his colleagues, in whom he placed much confidence, express their belief, that the cause of this state of things is fatty degeneration. Still this eminent pathologist does not agree with them, simply because he does not believe "that fat causes fragility of the heart." This latter opinion must now be entirely rejected. Moreover, Mr R. W. Smith (Dublin Journal of Med. Science. vol. ix., 1838) compares his own specimens of fatty degeneration to this illustration of M. Cruveilhier, which is, in truth, a good representation of the disease. Thus we have sufficient evidence to show, that very erroneous ideas have existed as to the nature of this softened state of heart, and that our present view of it is likely to prove the correct one. It is curious to observe that Corvisart (Compend. de Méd. Pratiq., tom. ii., p. 370), who states that he had never seen a case of fatty degeneration of the heart, in describing pale softening, suggests that this probably has been on a superficial examination confounded with fatty degeneration,

I am not aware that any chemical analysis has been made of any of these degenerated hearts. It is not very necessary. The action of æther, and the refractive properties of the granules, readily testify to their nature. Moreover, the extent to which the heart is affected with the disease, rather than the degree thereof, is the measure of its effects: hence chemical analysis could give but little additional information on the subject beyond that above stated.

II.—Of Preceding Observations on these diseases.

1. *Fatty growth on the heart.*—The older writers appear to have recognised many of the effects which have since been found connected with this condition of the heart. For example: Hippocrates,* in one of his Aphorisms, quaintly says, "Those who are naturally very fat are apt to die earlier than those who are slender." Celsus† pointedly refers to the difficulty of breathing and sudden death to which such individuals are liable.

These effects appear, however, not to have been traced to the state of the heart in particular, a diminished capacity of the vessels for the reception of the *pneuma*, or vital spirit, being the explanation given by Galen‡ and subsequent writers. The discovery of the circulation of the blood, and the evident necessity for the free and unembarrassed action of the heart, directed attention to the state of this organ in these and similar cases. Harvey§ himself, in recording the appearances found in the body of Parr, describes the heart as being much covered by fat. He did not, however, as incorrectly quoted by Schœffer,|| attribute the old man's death to this cause. In nearly all the systematic writers on the morbid anatomy of the heart, from the age of Harvey to the present time, cases and references illustrative of the effects of

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the general appearance of which it resembles. The use of the microscope has now reversed Corvisart's idea; for in all the flabby hearts which I have thus examined, fatty degeneration has been found.

* Genuine Works of Hippocrates. By Francis Adams, LL.D., Surgeon. Sydenham Soc.'s edit., vol. ii, p. 713.

† De Re Medica, lib. ii, s. 1.

‡ See the Commentary of Dr Adams; The Genuine Works, &c.; and Methodus Medendi, lib. xiv., c. 15.

§ The Works of William Harvey, M.D. By R. Willis, M.D.; Sydenham Soc.'s edit., p. 590.

|| Nova Acta Acad. Nat. Curios., 1761, tom. ii, p. 107.

the accumulation of fat on the heart will be found. Amongst these writers may be named Bonetus,[*] Lancisi,[†] Senac,[‡] Morgagni,[§] Lieutaud,[‖] Fothergill,[¶] Portal,[**] Parry,[††] and nearly all those who have written on the heart since the commencement of the present century. Illustrative examples, recorded by some of these writers, will be found in the Third Series of Collected Cases which accompany this communication. The fat accumulated on the heart is supposed to act mechanically, and by its pressure on the muscular fibres, on the nerves, and on the blood-vessels, to impede its function, embarrass its nutrition, and produce those effects which we shall subsequently have occasion to describe.

2. *Fatty degeneration of the heart.*—The knowledge which we possess of the nature of this important disease is of a comparatively recent date. Lancisi, whose writings on the heart, and on various subjects connected with hygiene and pathology, are deserving of more attention than they receive, evidently recognised and described this condition, when he said of fat particles, "sub formâ cinerearum macularum sparsim internas tunicas variegant."[‡‡] The "ash-coloured spots," which are now described as characterising the disease, had evidently attracted his attention. Lancisi, however, did not look on this condition as essentially morbid. Morgagni,[§§] more nearly approaches a correct estimate of its pathological effects, when, in describing the fatty condition of the heart in an aged female who had died from rupture of the organ, he asks, "Does the fat . . . . preserve the fibres in the vessels, and their contexture very lax, and make them give less resistance to the distraction from each other, if any considerable distension be brought on?" To our countrymen belongs the credit of having first directed attention to the nature of the disease. Dr Andrew Duncan, jun., in an interesting communication on certain Diseases of the Heart, published in the "Edinburgh Medical and Surgical Journal" in 1816, asks, "why the muscles of the heart should not become the seat of fatty degeneration as muscles in other parts of the body?" and then describes the appearance of a heart which he believed to be in this state. Dr Cheyne,[‖‖] of Dublin, soon after recorded in the 'Dublin Hospital Reports,' a similar case, and referred to the "only observation"

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[*] Sepulchretum; Lugd., 1700, lib. ii, § xi, Obs. 16, 17.

[†] De Subit. Mortibus, Romæ, 1707, p. 176.

[‡] Traité de la Structure, &c., 1774, vol. i, c. 4.

[§] De Sedibus et Causis, &c., 1765, Epis. 3, 27, 35.

[‖] Hist. Anat. Med., 1767, vol. ii, p. 19.

[¶] Medical Observations and Inquiries. London, 1776, vol. 5, p. 239, 247, 254.

[**] Memoires de l'Académie des Sciences. Paris, 1784.

[††] On Syncope Anginosa, 1799, p. 7, &c.

[‡‡] De Motu Cordis; Romæ, 1728, p. 55.

[§§] De Sedibus et Causis, &c., Epistola 27, Obs. 2.

[‖‖] Vol. ii, 1818.

ously published, that of Dr Duncan. Several cases were equently contributed to the same journal by Mr Adams.[*] specimens taken from these cases I had recently an opportu- of examining, through the courtesy of Mr Adams. The ect seems to have been felt as one of peculiar interest by Dublin School, and many illustrations have been furnished he researches of Dr Townsend,[†] Mr Smith,[‡] Dr Stokes,[§] more recently by those of Dr Bellingham,[‖] and Dr H. nedy.[¶] On the Continent we find Corvisart[**] stating, pro- ly a little before the period at which Dr Duncan's observation published, that he had not himself seen what was called fatty eneration of the heart; but as he had heard of other observers ing done so, some of the cases would probably soon be pub- ed.

aennec,[††] in his work on the Diseases of the Chest, devotes apter to "fatty diseases of the heart." In it he describes the umulation of fat on the heart and amongst its fibres. In these es, the fibres, he says, "may become atrophied and replaced fat, so that the columnæ carneæ seem held together by the ing membrane only." Still this, he adds, "is not the condi- n which I wish to denote by the name of *Fatty degeneration the heart.*" "This is an actual transformation of the muscu- substance into a substance possessing all the chemical and ysical properties of fat." The natural red colour of the heart then replaced by a pale yellow, like that of a dead leaf; the ssue is soft and greases paper." Laennec here evidently refers the true degeneration of the fibre, though he does not attempt show the nature of the change, beyond comparing it "to the tty degeneration of the voluntary muscles described by Haller ad Vicq D'Azyr."[‡‡] In fact, as fatty degeneration of the liver was long recognised before the actual changes in the tissue were described by Mr Bowman,* so this fatty degeneration of the heart had been recognised by Laennec and the writers above named, before its true character was ascertained. Laennec, however, was wrong in saying that the disease proceeded from without inwards, and also that it was confined to the apex of the heart.

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[*] Vol. iv, 1827.

[†] Dublin Journal, 1832, vol. i, p. 165.

[‡] Ibid., 1838, vol. ix.

[§] Ibid., 1846, vol. i, N. S., p. 491.

[‖] Dublin Medical Press, vol. xxi, 1849.

[¶] Ibid., vol. xxii, 1849.

[**] Maladies du Cœur, Paris; and A Treatise on Diseases of the Heart, &c. ranslated by Hebb. London, 1812, p. 166.

[††] On Diseases of the Chest, 4th edit., p. 583. Translated by Dr Forbes.

[‡‡] Fatty degeneration of voluntary muscles has long attracted the attention of atomists, and its characters in connection with those of the disease in question ere claim our notice. This change is observed in the muscles of the aged, in nused and paralysed muscles; it seems to occur also occasionally spontaneously, without an evident cause, in the muscles of both man and the lower animals. icq D'Azyr (Œuvres, par Moreau, tom. v, p. 365) has given in detail the results an examination made of a paralysed lower extremity, and he refers to the ob- rvations of Aristotle, Albinus, Leeuwenhoek, Haller, and Thual on the subject. e found the fat cells situated not between the lamina *(les lames)*, but in the ele- ents of the fibre; the fibres themselves being replaced by others thinner, finer,

MM. Bouillaud,† Andral,‡ Grisolle,§ and other French writers seem to have adopted the views of Laennec, and to have compared this change, without inquiry, to the fatty degeneration of the voluntary muscles. The increasing accuracy required in pathological investigations, and the improved appliances for conducting them, soon rendered this ambiguous term of comparison

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and more analogous to ligamentous tissue. I have examined this so-called fatty degeneration of voluntary muscles in four cases.

1st. The fatty muscle of over-fed prize cattle,—that which Vicq D'Azyr states that Aristotle described in the 'Historia Animalium.' "*Vertitur,*" he says, "*in pingue quoties pabuli copia suppetit.*" The muscular fibres were present in this instance nearly unchanged, but everywhere covered with fat cells, which lay in rows upon the fibre.

2d. The muscles of a paralysed youth, whose case is described by Mr Partridge (Med. Gazette, Nov. 1847, p. 944). Drawings, illustrative of the appearance shown in this case are presented. The fibres are shown in one overwhelmed with fat; in the other the fibres remain apparently very little altered, the fat having been removed by ether.

3d. The lumbar muscles of a paralysed sheep.—The preparation had long been kept in a closed bottle. (See some Account of an uncommon Appearance in the Flesh of a Sheep. By Dr Vaughan. London 1813.) The soft waxy substance was composed chiefly of fat cells, of obscure granular fibres, and of other fibres composed of delicate filamentous, but strong tissue; and, lastly, where the disease had least advanced, the muscular fibres remained.

4th. The intercostal muscles taken from between the ribs of a man.—The ribs had, for a long time, been rendered fixed by an accidental injury. I found the fat cells very abundant, and the striated muscles in many points. In other parts, the fine filamentous tissue seemed to have replaced the muscular fibres. There were also some granular fibres.

From these observations I am disposed to think, that the tendency of voluntary muscles is to degenerate into a fibrous tissue mixed with fat, rather than into granular fatty matter. This observation applies to paralysed muscles; it is not improbable, that in the flabby muscles of those suffering from scurvy and other diseases of mal-nutrition, a change similar to fatty degeneration of the heart may be found. We have, however, in the heart itself a change analogous to the fibrous degeneration just described. The tissue, then, is of a fawn colour, has a firm leathery feel, and in it we find fibrous tissue, fat cells, and muscular fibres. Dr Williams has described this fibrous degeneration M. Rokitansky has also described this change, but he regards the fibrous tissue as unstriated muscular fibre which has been developed amongst the ordinary fibres of the heart. For further information on the fatty degeneration of voluntary muscles, consult Beclard (Anat. Générale, § 168), Mr Paget's Lectures (Med. Gazette, vol. ii, 1847), and Mr Hallett (Edinb. Med. and Surg. Journal, 1848, vol. lxix., p. 30); also Idem. 1849, vol. lxxi, p. 257.)

* Lancet, vol. i, 1842.

† Malad. du Cœur, vol. ii, p. 294.

‡ Anatomie Pathologique, 1820, tom. ii, p. 318.

§ Traité de Pathologie Interne, vol. ii, p. 337.

unsatisfactory. Hence the seat of the fatty matter, and its relation to the tissue in which it was found, were examined. Dr Williams,* whose name I cannot mention here without a respectful tribute of thankfulness, for the valuable assistance and advice which he has given me during the progress of this inquiry, as on many other occasions, states in his work on Diseases of the Chest, published in 1840, that in true "sebaceous degeneration, the fatty matter is deposited in, and not on, the muscle of the organ, which thereby becomes changed in nature, and impaired in functions." Thus is marked the first step in this inquiry. In the same year, some cases fell under the notice of Dr Peacock, with the notes of which he has kindly favoured me, and which were subsequently published in the Monthly Journal of Medical Science, for 1844. In these cases, Dr Peacock describes the tissue of the heart as being pale, soft, mottled, and greasy, resembling fatty liver. The change he describes as being most evident in the interior of the heart,† the organ being destitute of fat on the surface. The microscopic examination of the tissue, showed the fibres almost or entirely deprived of their transverse striæ, and covered with small oil globules. M. Rokitansky's‡ more extended investigations have added greatly to our knowledge on the subject. He describes three forms of fatty disease: 1st, excessive accumulation on the surface; 2d, intrusion of fat amongst the fibres, constituting what he calls fatty metamorphosis; and 3d, degeneration of the fibre, which presents varieties, according to the extent, seat, and degree of the evil. M. Rokitansky, however, left much to be added, and something to be corrected. To Mr Paget§ the English Pathological School is indebted, for his introduction to it of the views of the last named writer, but still more for his own more accurate and more extended investigation into the nature and anatomical characters of this morbid process; to which he has added some illustrations of one of the most striking of its effects, viz., sudden death. Dr Ormerod|| has recently followed the footsteps of his learned colleague, and greatly enhanced our knowledge of the subject, by a series of cases exhibiting the variety of circumstances under which this disease may occur. In addition to the preceding individual cases of considerable interest have been recorded by Mr Mead,¶ Dr Hughes,

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* The Pathology and Diagnosis of the Diseases of the Chest; London, 1840, p. 279.

† The presence of fat in the internal portions of the heart's texture has been described by Dr Elliotson, (Lect. on Med., London, 1839, p. 36; and Diseases of the Heart, p. 32.)

‡ Handbuch der Pathologischen Anatomie. Von Carl Rokitansky. Wien, 1842. Band iii, p. 462.

§ Medical Gazette, vol. ii, 1847, Lecture vi.

|| Ibid., vol. ii, 1849.

¶ Ibid., 1846.

Bennet,[*] Dr Latham,[†] Dr Crisp,[‡] Mr Stallard,[§] Mr Corfe,[‖] and other observers, abstracts of whose cases are given in the table.

Lastly, this disease has been observed amongst lower animals. Mr Kent[¶] has given a description of a rupture of the heart in a mare. The right ventricle and the auricle had undergone fatty degenerations, and old adhesions of the pericardium existed. During life, the animal had exhibited several of the symptoms which characterise the disease in man. I have also received the histories of two very similar cases: one, that of a fat ox; the other, of a London dairy cow; but it is not necessary to detail them now.

III.—A CONSIDERATION OF THE CIRCUMSTANCES UNDER WHICH FATTY DISEASES OF THE HEART TAKE PLACE,—THEIR CAUSES.

1. *Fatty Growth on the Heart.*—In every inquiry of this kind we are met by the unsolved problem, Why are certain individuals and certain parts of the body more prone to the formation of fat than others? We can ascertain, with some degree of certainty, the circumstances which will promote the formation of fat in general;[**] and, as the facts shown in the table tell us, that when fat forms throughout the system, the heart will partake largely of the accumulation, we must, therefore, at present content ourselves by recollecting what the nature of these influences of extended operation is. Thus, in the first instance, we know that the material of the fatty tissue must be derived, directly or indirectly, from the blood,[††] and that the quantity of the one must bear a relation to the supply of material from the other.

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[*] Edinb. Monthly Journal.

[†] Clinical Medicine. London, 1846, vol. ii, p. 169.

[‡] Trans., Path. Soc. of London, vol. i.

[§] Transactions of the Provincial Assoc., 1847, vol. iii, N. S., p. 105.

[‖] Medical Times, vol. i, 1848-49, p. 142.

[¶] Lancet, vol. i, 1847, p. 125; and Veterinary Record.

[**] See Obesity, by Dr Williams (Cyclopædia of Practical Medicine, London, 1833); and Adipose Tissue, by Dr Craigie (Cyclopædia of Anat. and Physiol., London, 1836). And since this essay was presented, Lectures on Obesity, by Dr T. K. Chambers, (Lancet, vol. i. 1850).

[†] All *healthy blood contains fat*, as shown by the researches of Chèvreul, Lecanu, Boudét, Trail, Christison, Babington, Davy, Simon, and others. It is found in the serum, in the fibrin, and in the globules. (See Cyclopædia of Anatomy, as quoted above, pp. 59–60; Simon's Chemistry, Sydenham Soc.'s edit. by Dr Day, 1845, vol. i. pp. 163, 177, 188, 195.) This fat is in a diffused or saponified form, and does not admit of recognition by mere examination, as in the case of milky-looking blood. It is said, however, that, in some of these cases of "fatty degeneration," the fat is so abundant in the blood, as to be seen floating in it like oil. Such is Mr R. W. Smith's description (Dublin Journal, vol. ix.) Dr Stokes (Idem, vol. i., N. S.), seems to confirm the observation. Haller (Elementa Physiol.) refers to Malpighi, Glisson, and Ruysch, as having observed free oil globules in the blood. Dr Gaie (Payne's Letters on the Cholera, p. 151), describes a similar appearance in the blood of persons dead of this disease. In making these observations, care must always be taken that the oil flowing from the divided tissue and mixing with the blood, does not become a source of error.

Secondly, We know that the quantity of fatty matter in the blood is in direct relation with that supplied by the food on the one hand,* and on the other with the greater or less freedom which exists for the free elimination of its elements in the respiratory process.† We therefore feel that if the food is deficient in the materials which readily constitute fat, and the respiratory process is accelerated and increased by abundant exercise, little fat can be formed. Equally true is the converse, that those who fare luxuriously and lead sedentary lives will grow fat. In the one case, neither the body nor the heart will suffer; in the other, in the general accumulation, the heart will certainly participate. Beyond these general principles, I fear we cannot go, and even to these there are exceptions.

Thus the table, series iii., shows, that of fifteen cases of extreme fatty growth on the heart, eleven occurred in very fat individuals. and only one in a person who is described as being thin. M. Bizot found nine fat hearts in fourteen fat females; but he also found fourteen fat hearts in twenty-nine thin females. In males, the numbers were still fewer. Such cases are illustrations of the exceptions referred to; and further illustrations, though they are somewhat more apparent than real, are found in cases of phthisis. To these it is not possible to refer now. We observe that age marks a decided influence over the formation of fat on the heart. It is very scanty in infancy, and is rarely present in any quantity before the thirtieth year.‡ Thus of the fifteen cases comprised in this series, thirteen were above fifty years of age, and one only under that age. Males, too, according to this collection of cases, are more liable to this excessive accumulation of fat than females, the numbers being as twelve to three. M. Bizot's observations as to sex are different from mine. He finds that females are more liable to have a large amount of fat on the heart than males; still, as his observations do not apply to this extreme condition, but to a mere greater or less quantity of fat, they can scarcely be said to be contradictory of those now enumerated.

2. *Fatty degeneration of the Fibre itself.*—In the following

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* For observations and experiments on the presence of *fat in the blood derived from the food,* see notes to Hewson's works by Mr Gulliver, Sydenham Soc.'s edit., 1846. p. 35; also Simon's Chemistry, vol. i. pp. 192 and 202 (in blood of *vena portæ*), and also more recent experiments by M. Millou (Med. Gazette, vol. i., 1850, p. 127).

† For information on the circumstances which influence *the elimination of carbon* in the process of respiration, see Simon's Chemistry, vol. i., p. 128; also on the elimination of fatty matter by the lungs, see Comptes Rendus (Juin. 1847), which contain some curious results as to the quantity of fatty matter obtained from the lungs according as the respiratory process is more or less free.

‡ Corvisart (Maladies du Cœur) quotes from Kerckring the case of a child whose heart seemed wanting, so great was the quantity of fat in which it was imbedded. 3ieme edition. Paris, 1818. Chapter iv., Art. iii., p. 180.

observations I seek to establish the fact, that the molecular fatty matter in the fibre is the result of a chemical or physical change in the composition of the muscular tissue itself, independent of those processes which we call vital.

Anatomists are well acquainted with the substance named adipocere, and with the facility with which it frequently forms in dead animal tissues excluded from the air and exposed to moisture. * Much difference of opinion has existed amongst chemists as to the origin of this substance. All admit the great quantity of fatty matter in its composition. One class, however, which includes Gay Lussac, and Berzelius, believes that "the compound results entirely from the fat originally present in the substance, and that the fibrin is completely destroyed by putrefaction." † The other class, which includes Dr Thomas Thomson ‡ and Mr Brande, believes "that the fatty matter is an actual product of the decay, and not merely an educt or residue." The correctness of the latter opinion is shown by the following observations. A piece of this substance, obtained from the muscular part of the thigh of a horse, (portions of the specimen are in the British Museum and in that of the Royal College of Surgeons,) was submitted to examination. In its general appearance it resembles spermaceti. It is rather darker coloured and more fragile. Its surface is marked by irregular outlines of a red colour, evidently the boundaries of the fasciculi of the muscular fibres; it emits a strong ammoniacal odour, floats on water, and is nearly all dissolved in ether. The ether, when allowed to evaporate, leaves a large quantity of granular and fatty matter.

The trifling undissolved residue presents, when floating in water, a delicate flocculent appearance. Examined with the microscope, the flocculent matter is found composed of delicate filaments forming a cellular web. In some parts the filaments presented something of a longitudinal arrangement, in others the web appeared perforated by regular foramina.

A microscopic section of the adipocere itself, unacted on by any agent, (the section being made with a knife slightly warmed, required by the fragility of the substance,) showed the following appearances.—" 1st, Bands running longitudinally, as in the arrangement of the muscular fibre. These bands are dark coloured and opaque, except at the broken edges or extremities;

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* Haller (Elemen. Physiol., p. 29), mentions the names of Ruysch, Beecher, Bruckman, and others, as familiar with this substance. Mr G. S. Gibbes, in a communication addressed to the Royal Society (Philos. Trans., 1794–95), directed attention to the formation of adipocere, and adds that Sir Thomas Brown, in his 'Hydriotaphia,' had described it, and also Lord Bacon in the 'Sylva Sylvarum.'

† See Adipocere, by Mr Brande, Cyclop. Anat. and Physiol., vol. i., p. 56.

‡ Annals of Philosophy, vol. ii., 1818. "The quantity of fatty matter, in this instance, was by far too great to suppose it to have pre-existed in the living body."

at these points they are seen to be composed of small crystalline scales. The wavy outlines of the blood-vessels or nerves are sometimes seen between the fibres or fasciculi. In the intervals may be observed flat discs composed of radiating acicular crystals. These appearances vanished on putting ether on the specimen, and the merest particle of the filamentous substance remained.

From these observations I conclude, that whatever be the nature of the process, the places of the muscular fibres, the blood-vessels and nerves are occupied by fatty matter, which could not have existed in them during life. This opinion is confirmed by experiments on what may be called the artificial formation of adipocere. Mr Gibbes, whose communications to the Royal Society have been just mentioned (note, page 134), states that Lord Bacon had indicated the mode of converting the waste flesh of animals into fat, which might be used for various domestic purposes. Mr Gibbes, acting on this idea, succeeded by different processes, including exposure to running water, to nitrous acid, &c., in converting large quantities of animal flesh, even the carcase of a whole cow, into fatty matter. The difficulty which the experimenter met with in freeing this substance from colour and smell, or his death, appears to have prevented the communication of his further proceedings, which he had promised to the Society, and neither his nor Lord Bacon's discoveries have been made useful in supplying fat from waste flesh. To the pathologist they afford information of peculiar interest in connection with the following experiments. A little more than two years ago I obtained, for the purpose of examining the healthy structure of the tissue, the healthy heart of a healthy child who had died a few hours previously from the shock of a severe burn. Having satisfied myself on the required point, I placed the specimen in weak spirit and water (one part to eight or nine), for the purpose of future examination. On looking at the specimen after a few weeks, I found it greatly changed; it had a confused granular aspect, to such an extent, indeed, as to lead me to feel, that there must be some error in previous observations on the specimens of true degeneration, as portions of a healthy heart now exhibited characters so similar. I found, however, that this change existed in all parts of a heart, which I had no doubt had been healthy, in some parts at least, when previously examined. I mentioned the observation to Dr Williams, and was gratified at hearing from him, that the fact of this change was an illustration of an experiment which he had suggested to his clinical assistant, Mr Edward Palmer, sometime previously, viz. to try whether fatty matter was not formed after death by a molecular change in animal tissues kept excluded from the air but exposed to moisture.[*] He suggested further observations of this specimen, and some further experiments. These have been made, and the result has been, that the heart first described presents the most marked an universal character of true fatty degeneration. It is of a pale cream colour, it is soft, portions of it float in water, and when examined with the microscope exhibit the most advanced state of degeneration, differing from that which occurs during life in this respect alone,—that it is universal. Every fibre is fatty, whilst in that degeneration, which occurs during life; many fibres retain their healthy organization.

A specimen of a sound sheep's heart, placed in very diluted nitric acid,[†] has been traced through a series of similar changes; but as a shorter interval has elapsed, it has reached a less advanced stage. The specimens have been examined by Dr Williams, Mr Quekett, and Mr Toynbee,[‡] and others. Illustrations of the appearances presented will be found in Plate V., fig. 1. The blood-vessels and nerves participate in this change. It does not appear necessary to illustrate further the conversion of these tissues after death, external to the body, into fatty matter,[§]—we shall therefore turn to inquire whether like changes do not occur within the body and before death. Dr Babington[‖] has apparently observed the formation of fatty matter in the blood at the expense of the albumen. Dr Williams, in the first edition of his 'Principles of Medicine,'[¶] recognises the presence of "fat in softened tubercle, pus, atheroma, and gangrene of the lungs, as being derived from the debris of animal matter, as in the conversion of flesh into adipocerous matter." In a subsequent edition of the same valuable work,[**] further illustrations of this process, and of the circumstances under which it occurs are clearly given. M. Rokitansky* mentions several instances in which fat may be found (as in masses of fibrin, in non-vascular tumours, in the walls of arteries, &c.), in situations in which its presence could not be deemed the result of a deposition from the blood. Mr Paget mentions† a striking example of this change observed by him in some masses of what had been medullary cancer of the liver, "certain parts of which, when examined, were found to consist almost entirely of oil globules." I have seen the same appearance in the central softened portion of a large mass of tubercle from the kidney, the fatty globules being there infinitely more numerous than in those parts where the softening had not commenced. Dr Copland has recently communicated to me the particulars of a case of chronic inflammation of the membranes covering the spinal cord, which caused paraplegia. The patient finally died from the paralysis extending to the muscles of respiration. After death, the recent effusion of plastic matter which existed on the upper part of the cord was found to pass inferiorly where the inflammation had first commenced and ceased into a soft fatty matter. The same learned authority mentioned to me, at the same time, another case, in which the central portions of some large bands of adhesions, found in a case of chronic peritonitis, consisted of fat. I find a case recorded, without particular reference to this point, by Dr Archibald Hall,‡ in which the fibrin of a hæmorrhagic effusion into the *theca vertebralis*, consequent on a fall, was found, after an illness of some months' duration, changed into fat.§ The fatty matter here formed, independently of a direct connection with the vascular system, is, at least in such of the cases as have been examined with the microscope, and they have all been, but the two last-mentioned, found to be, in all its characters, analogous to the granular fatty matter observed, not only in degenerated muscular fibre, but in various other organs which are the seat of fatty degeneration. Here, then, a most important question arises,—Is the fatty matter which is found in the degenerated muscle of the heart derived from a molecular change in the texture, or is the fat deposited from the blood? In answer to this question, we refer to the preceding facts. We have seen the substances analogous to muscle in composition converted, both in and out of the body, into this same fatty matter. We have traced also the muscular fibre itself undergoing (external to the body, and thus beyond the reach of deposition, or the influence of the nutritive process), a change perfectly identical with that which it undergoes in the living body. There is, therefore, a presumption in favour of the view which holds that the processes in both cases are identical; that, in fact, when these protein compounds, albumen and fibrin, are effused in a form which is not readily susceptible of organization, when even they enter into the composition of tissues and textures, the organization of which is imperfect from inherent depravity, from natural decay, or from a deficient supply of those elements and influences, in the absence of which nutrition fails, that in these cases, I say, those substances degenerate and pass into fatty matter. In other words, that when the vital power which belongs to these higher products of animal organization is weakened or destroyed, they yield to the physical and chemical influences which surround them, and, by an inherent principle, descend into a class which is shared by them in common with plants and minerals.* It would be highly interesting to illustrate this argument by facts drawn from the multitude of instances in which different observers have recently found this fatty degeneration. Some of these facts, no doubt, readily suggest themselves, though perhaps less in the case of the heart than in other organs. It is this organ, however, which claims our attention here. An interesting observation, made by Mr Paget, supplies a most important step in this inquiry. It is now pretty well established that a cell germ, or nucleated cell, is an essential element in the nutritive process of all tissues. The first change that can be traced in the process of degeneration of the heart's fibres, and it requires care to observe it, is the destruction of these cell-germs in the tissue. Mr Paget† writes, "When the change is least marked, and but little fatty matter has collected within the sarcolemma, the outlines of the nuclei look dim, and they lose their colour. In the further advanced stage, the nucleus of the fibre cannot be seen at all; its former place is indicated, if at all, only by some out of the narrow column of yellow granules, and, in a yet later stage, or when the sarcolemma appears nearly full of fatty particles, all trace of both the nucleus and these granules is lost."

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[*] The same idea has been entertained and clearly expressed by Dr Hodgkin in the Seventh Report of the British Association, and will also be found in his 'Lectures on the Morbid Anatomy of the Serous and Mucous Membranes,' vol. ii., part i., 1840, p. 539. The fact of finding a fatty liver in a preparation which did not seem to be such when taken from the body, contributed to the formation of this view.

[†] Mr Brande (*loc, cit.*) states, that "the action of very diluted nitric acid on some modifications of albumen, is also attended by their conversion into an adipocerous substance."

[‡] Mr Paget also kindly examined these specimens shortly before this communication was read.

[§] Mr Gulliver (*loc. cit.*) mentions examples of the apparent conversion of albuminous matter into oil, as being noticed by Sir H. Davy, Dr Davy, and himself. Wurtz has formed butyric acid from the decomposition of fibrin (Simon's Chemistry, vol. i., p. 79); and it is stated in Dr Garrod's Lectures in the 'Lancet,' that Guckelberger has found the protein textures capable of affording, by their decomposition, a variety of compounds similar to those which belong to the amylaceous series.

[‖] Cyclopædia of Anatomy and Physiology, vol. i., 1836, p. 423.

[¶] London, 1843, p. 321.

[**] Second edit., 1848, p. 371 *et seq.*

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* *Op. cit.*, band i., Wien, 1846; and British and Foreign Med.-Chirur. Review, vol. i., 1848, p, 169.

† Lectures, *loc. cit.*, p. 149.

‡ British American Journal of Medical Science, vol. iv. p. 61.

§ Senac (Traité de la Structure, quoted in Dr. A. Duncan's Essay) describes a case from Weitbrecht, in which a substance resembling lard occupied the pericardium, the result, apparently, of this fatty change in effused fibrin.

Thus we have evidence that the nutrition of the heart is impaired, and that those powers are weakened, by which it is enabled to resist the influences which tend to the disintegration of its tissue. A review of the circumstances under which this degeneration occurs, is further confirmatory of the view adopted here. These circumstances exhibit impairment of general and local nutrition.

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* The origin of the valuable hydrocarburet—coal from vegetable matter, suggests something very analogous, in the transformation which occurs, to the formation of adipocere from animal matter.

† Lectures, *loc. cit.*, p. 145.

1. *Fatty degeneration dependent on general or constitutional causes*, is found in persons who have suffered from a variety of exhausting diseases. Amongst the cases recorded by Dr Ormerod,[*] some striking illustrations of this will be found. Of twenty-five cases, three had suffered from hemorrhage, three from phthisis, one from delirium tremens, one from cancer, two from fever. These cases are taken chiefly from hospital practice, and illustrate better than the cases which I have collected, and which are derived from different sources, the relation of this disease to general impairment of nutrition; still amongst my cases will be found several similar to those now mentioned. Thus, one is that of a child, who, having been long in bad health, died of cancrum oris; another is that of a young woman, who had suffered from long-continued gastric irritation and chronic phthisis; a third from hæmaturia. Several other examples, in which the nutritive functions were equally impaired, will be found amongst the cases presented with this communication, and which it is unnecessary therefore to refer to here. But besides this class of cases, there is another, in which we find degenerations going on simultaneously in other organs, for example in the walls of arteries, in the liver, in the kidneys, &c., giving evidence, likewise, of an imperfect nutritive function, acting widely, and shared in by the heart.[†] Of this state, illustrations, which more careful observation might have rendered more numerous, will be found in the tables. This degeneration of arteries, which Mr Gulliver has long since shown to be fatty, has already been recognised by Dr Bellingham, as being frequently associated in the form of aneurism, with this analogous condition of the heart.[‡] And it is of considerable interest in connection with the second condition, viz. :

2. *Fatty degeneration connected chiefly with a local modification of nutrition.* In thirteen of the thirty-three cases of degeneration of the heart (Series I.) the coronary arteries were more or less ossified or obstructed, likewise in seven of the second series, and in five of the third.

In all the cases which I have myself examined on this point (except in a case of phthisis, and some cases allied with it), I have found more or less obstruction of these vessels. I have seen the coronary artery extremely ossified, going directly to the only part of the heart affected. (See Case 26, Series I.) A nearly similar condition will be found reported as having existed in several other cases in both series. At least, arteries proceeding to the principal seat of disease are found more or less obstructed. (See Cases 9 and 26, Series I; and Cases 2 and 3, Series II.) In the last case, the coronary artery was single and ossified. This connection between fatty softened heart and obstructed arteries suggests an analogy with softening of the brain, in which a like condition of the vessel is known to exist.[*]

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[*] Medical Gazette, vol. ii., 1849.

[†] The simultaneous existence of the *arcus senilis*, or fatty degeneration of the cornea, with this affection of the heart, will be subsequently referred to as an aid to the diagnosis of the latter.

[‡] Dublin Medical Press, vol. xxi., 1849, p. 290.

Dr Abercrombie[†] has, indeed, compared this cerebral softening to dry gangrene, and he was not wrong, when the difference in the relation of the parts to external influences is remembered.

There are two causes which contribute in a marked degree to the frequent association of diseased arteries with fatty degeneration of the heart:[‡] one of these is, that the disease of the arteries is itself an evidence of the existence of a more or less impaired state of nutrition throughout the system. The other is a fact mentioned by Mr Swan,[§] in a recent communication on the Blood-vessels of the Heart. It is, that there is not a free communication between the coronary arteries; that one of them cannot be readily injected from the other, hence obstructions from any cause in one, will not admit of the deficiency being compensated for by the supply from the other. Another local modification of nutrition, caused by the previous occurrence of endo- or pericarditis, is found connected with fatty degeneration. Dr Williams[‖] has long since observed this association; he says, "a pallid yellowish appearance of the substance due to an altered state of nutrition, is not at all an uncommon accompaniment of other lesions of the heart, such as accumulation of fat and adhesion of the pericardium connected with partial obstruction of the coronary arteries."

M. Rokitansky goes so far on this point as to say, that this degeneration "most frequently occurs in hypertrophied hearts, in combination with the remains of endo- and pericarditis." And here it might be stated, that this author[¶] believes the fatty degeneration to depend on an impairment of the nervous power of e heart. In hypertrophied hearts he says, " the balance is lost tween the nervous powers and the volume of the muscles; erefore, in those parts where a paralysis, so to speak, exists, this ange occurs, as in the muscles of animal life."

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[*] Since the presentation of this communication to the Society, a highly interesting observation has been published by Mr Paget, showing that fatty degeneration of the small cerebral arteries exists in many cases of cerebral apoplexy.

[†] Diseases of the Brain.—Edinb. 128, pp. 25 and 269.

[‡] M. Bizot finds ossification of the coronary arteries more frequently in males than females, and rarely before 40 years of age. These facts correspond as to the sex in, and age at, which fatty degeneration most frequently occurs. He likewise found the left coronary artery more frequently ossified than the right, as the left ventricle is also more frequently the seat of degeneration than the right.

[§] Medical Gazette, vol. xlii, p. 751.

[‖] The Pathology and Diagnosis of Diseases of the Chest, 1840, p. 245.

[¶] Loc. cit., p. 463.

This eminent pathologist seems to have overlooked a fact, hich will presently be further illustrated, that the disease occurs n hearts which are unchanged, or actually diminished, in size; nd secondly, if his idea were correct, all hypertrophied muscles, in which a similar loss of balance occurs, must, on the same principle, suffer in the same way. It is well known that they do not do so.

Mr Paget* pointedly describes the effects of inflammation in producing atrophy and degeneration, not only of the heart, but also of the kidneys, liver, &c.

The mode in which the inflammation acts in these cases will be found also fully discussed by Dr Williams† in his " Principles of Medicine," under the head " Degenerations."

Endo- or pericarditis had previously existed in six cases in Series I., and in eleven in Series II. Lastly, it appears, that in some cases dilatation of the right side of the heart, and congestion of the coronary veins, may promote degeneration of the fibres. Thus, then, we trace the diseased state of the heart's texture to a variety of causes, all of which appear to act in the same way; that is by interfering with the supply from the blood, either in quantity or quality of those elements, which are essential to the healthy nutrition of this organ, and which should, no doubt, be in relation to the high degree of organization and vitality which its peculiar functions require. These disturbing influences we trace in all those too-familiar agencies which produce impaired health, and premature decay, as likewise in the progress of life, from natural decay. A variety of circumstances connected with the origin and progress of this disease, including the diseased condition, with which it is observed to be connected, will be found in the Tables of Cases and the accompanying Abstract To recapitulate these circumstances here would be tedious and unnecessary; it will suffice to say that, in reference to ex, the disease is more frequent in males than females, in the proportion of twenty-four to nine (in Dr Ormerod's cases the roportions are similar, being fifteen to seven). In reference to ge, one individual only is under 20 years of age, four are between 20 and 30, the same number between 30 and 40, and fifteen, or early half, are over 60. Twelve of thirty-three cases are described as having occurred in fat individuals, and nine in those who were thin. And here it would be well to remark, that the quantity of fat on the heart itself was more than usual in twenty-one cases of thirty-three, and less than usual in four, being nearly absent in three of these. In reference to the class in society, in which the disease occurred, nine of thirty-three belonged to the higher ranks, eight to the middle, and sixteen to the lower; whereas, in mere fatty growth on the heart, the proportions are reversed: seven of fifteen belonged to the first class, six to the second, and only two to the third. Fatty degeneration and fatty growths thus contrasted are seen, in a great degree, to arise from different causes, the one is the result of an accumulation in the blood of the elements of fat, the other is the result of decay and disintegration. Fatty growth may occur alone, so may fatty disintegration; but as both are the results of a degraded and bad nutrition, both may be often found in combination.

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* Lectures, loc. cit., p. 148.

† Principles of Medicine, 2d edition.

IV.—The Effects of Fatty Diseases on the Structure and Functions of the Heart.

1. *Of fatty growth.* In a preceding part of this communication, the opinions which have been held as to the effects of fatty growth in excess on the heart, have been mentioned. These effects may be briefly enumerated, as being languid and feeble circulation,* a sense of uneasiness and oppression in the chest, embarrassment and distress in breathing, coma, syncope, angina pectoris, sudden death, rupture of the heart, &c. Thus amongst fifteen cases, difficulty of breathing was present in nine; syncope or faintness in eight; giddiness and coma in five; pain in the region of the heart existed in six; and in four of these, amounted to angina pectoris. Of the fifteen cases, fourteen died suddenly, of which ten may be said to have died by syncope, three by rupture of the heart, one by coma; one died by disease of other organs. This catalogue of evils, in a series of cases in which no other lesion of the heart's texture is stated to exist, not even softening, save simple accumulation of fat, would suffice to place this in a prominent position as the source of very serious results. But we are in this difficulty, that we are not quite sure that, in these cases, some unrecognised degeneration of the fibres did not co-exist. We shall now see, that in this latter condition, these effects occur in an equally striking degree; it will, therefore, be well not to dwell too much on such effects, as proceeding from fatty growth alone.

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* The impaired contractile power of the heart, and the immediate source on which this depends, cannot be more clearly expressed than in the following words of Haller, when describing the condition of the voluntary muscles in a state of "fatty degeneration." He says, "Deinde inter fibras musculosas congestus eas a se invicem ita separat ut pene evanescant, et ex parallello situ divulsæ, inque nimis molli medio natantes vim suam contractilem desinant exercere."—Elem. Physiol., lib. i, § 4.

2. *Fatty degeneration.* The effects of fatty degeneration of the fibre on the structure and function of the heart might almost, *à priori,* be inferred, when it is recollected, that the firm contractile muscular flesh is more or less replaced by an inert fatty matter, and that this change represents, at the same time, impaired constitutional powers. All these effects may be comprised under these heads: *a,* Impairment of structure; *b.* Impairment of function. Many of these effects being capable of recognition during life, may be said to be symptomatic of the disease.*

A. *Effects on the structure of the heart.—a. The size.* That hypertrophy of the heart and fatty degeneration are frequently associated, has been already mentioned. This condition is recorded as being present in twenty-three of thirty-three cases in Series I., and in sixteen of thirty-five in Series II. The size of the heart is stated to have been unchanged in eight cases of these two series, and to have been diminished below the natural standard in four. It is, therefore, perfectly clear, that fatty degeneration frequently occurs in hypertrophied hearts; not, I should say, as M. Rokitansky supposed, from a disturbance in the balance of nervous power,† but from these enlarged hearts requiring a larger supply of the materials for nutrition, which we have already seen so materially interfered with, or from the elements of which these enlarged hearts are composed, being in themselves more susceptible of change. It may be worth suggesting, whether the hypertrophy may not, in some of these cases, be the result of a natural effort to compensate for loss of power. In one case (No. 4, Series I.), the heart weighed twenty-two ounces, and there was no obstruction, nor other disease, save this degeneration, which could account for the hypertrophy. The heart is said to have been dilated in six cases of Series I., in three of these in association with hypertrophy. The heart retaining its size, or being less than natural, does not call for special remark.

b. The texture. The softness and fragility of the heart's tissue have already been described. They are the source of some of the severest and most fatal lesions to which the organ is liable, viz., rupture. This lesion will be found recorded in eight cases of thirty-three in Series I., and in seventeen of thirty-five in Series II.;* or in twenty-five cases in sixty-eight. The rupture was complete, that is to say, it perforated the walls of the heart, so as to permit hæmorrhage into the pericardium in all the cases except six. One of these cases was a rupture of the septum of the ventricles; in two, there existed ruptures within the substance of the wall of the left; in one, within that of the right ventricle; in one, there was a partial rupture of the internal fibres of the right auricle; and in another, a partial rupture of some fibres on the external surface of the heart. All these cases were fatal within a short period after the accident was supposed to have occurred. The narratives of the cases suggest many points of interest in connection with this lesion, which may, on a future occasion, occupy attention. It will suffice now to say, that from the time when Harvey adduced a case of rupture of the ventricle, and the effusion of blood into the pericardium, as proofs of his then much doubted theory, this accident has attracted the notice of many pathologists, and its cause has received a variety of explanations. The actual friability of the heart appears to be quite sufficient to account for the lesion in any case in which fatty degeneration can be found; and that degeneration, I find good reason for believing, exists in the greater number, if not all, the specimens in museums which I have examined, and likewise in very many of the recorded cases of this accident, besides those from which the preceding numbers are taken. Amongst the lesions resulting from a partial rupture, are some of which claim separate mention. One of them is the appearance described by Cruveilhier,† as cardiac apoplexy. (Case 2, Series II.), and caused by hemorrhage into a portion of the heart's walls. One of the cases in the Table is a good illustration of this form of hemorrhage into the wall of the right ventricle, a situation in which M. Cruveilhier states it never occurs. (See Case 15, Series I.) An extremely interesting example of this disease has been recorded by Mr Stallard,‡ (Case 14, Series I.) The case recorded by Mr Stallard affords also an illustration of another appearance produced by this partial rupture and hemorrhage. It is when the accident has not been immediately fatal, the coagulum loses its colour, producing an appearance like an encysted abscess in the walls of the heart. These appearances have been fully noticed and described by Meriadec Laennec.[*] He refers to the "ingenious opinion," as he calls it, of M. Cruveilhier, that consecutive false aneurism of the heart may result, as no doubt it in some cases does, from these so-called abscesses communicating with the ventricle. The origin of aneurism of the heart is unquestionably often connected with degeneration. It may be formed in the mode just described, or by a portion of the heart more soft than the rest yielding to pressure, and bulging outwards.[†]

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* It should be stated, that many cases are recorded, in which patients have died, and in whose hearts this condition has been unexpectedly found. Some of these cases are said to have been previously in the enjoyment of perfect health; others have been ailing, but not in connection with the heart; others ailing likewise, and the heart being suspected has been examined, and failed to give evidence of the disease, which post-mortem examination has subsequently shown to be present. I have not met with such cases. On the contrary; even in some instances in which the individual dying suddenly was said to be in good health, I have found, on inquiry, that symptoms, more or less marked, had existed, and might have been previously recognized, if attention had been called to them.

† The only case in which I have found disease of the heart stated to be connected with disease of the nerves, is a case of softened heart, mentioned by Joseph Frank (Praxeos, Med. Univ. Præcepta, vol viii., part ii., § liv., p. 316; and Encyclographie des Sciences Méd., vol. viii., p. 188), in which the vagus nerves were diseased.

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* This disproportion in these series is due to the fact, that the cases in Series I. have been taken as they occurred; whilst those in Series II. have generally been recorded in consequence of some peculiarity. Such, for example, as this very lesion.

† Anat. Pathol., vol. ii., Maladies du Cœur, and livr. iii., pl. i.

‡ Trans. Prov. Med. and Surg. Association, N. S., vol. iii., 1847, p. 105.

Four examples of this lesion, besides that first mentioned, will be found amongst the cases recorded here, (viz., 7, 19, and 27, Series I.; and 25, Series II.) Leaving these textural changes for the present, we must now pass on to consider;

B. *The effects of fatty degeneration on the functions of the heart.*[‡] The most prominent of these effects are those which exhibit the deficient powers of the organ.

a. Coma.—Several writers have described coma, preceded or not by giddiness in connection with enfeebled powers of the circulation. Mr Adams,[§] whose cases have been previously mentioned, observed as many as twenty attacks of coma in one of them. Mr R. W. Smith and Dr Stokes[||] have made similar observations. The explanation of these attacks is this, that the power of the heart is reduced, and thus it is rendered incapable of readily sending on the blood which it receives; hence arises obstruction to the circulation and unequal pressure on the brain. The explanation is no doubt correct. Case 32, Series I., may be received as a confirmatory illustration. In this case there was much degeneration and fatty growth on the right side of the heart, the left being comparatively healthy. The right cavities were filled with blood. The patient, an old lady, had died from meningeal apoplexy, consequent, apparently, on the obstruction to the free return of blood from the brain. In the Table, four of the first, and two of the second, series of cases died by coma.

b. Syncope.—Cardiac syncope is a term properly and more *frequently* used by the older writers[¶] than by ourselves, notwithstanding the arguments of Bichat[**] on the subject. Dr Burrows,[††] in his interesting work on the Cerebral Circulation, has clearly established the fact, that syncope is due to a deficiency of that pressure within the skull which is essential to the performance of the functions of the brain. On this principle we can explain the frequency with which this symptom is found to occur in fatty disease of the heart. Numerous illustrations of the fact will be found in the Tables.

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[*] Diseases of the Chest. Translated by Dr Forbes, 1834, note, p. 581.

[†] As described by Dr Latham, loc. cit.

[‡] Dublin Hospital Reports, vol. iv.

[§] Ibid.

[||] Dublin Journal, vol. i., N. S., p. 491.

[¶] Boneti Sepulchretum, &c.

[**] Sur la Vie et la Mort, art. 5.

[††] On Disorders of the Cerebral Circulation. London, 1846.

The case of an old man who fell under the notice of Dr Williams and myself is very remarkable. I was one day suddenly called to him, and found him, as I myself and others believed, dead. He had been in his usual state, and taking his dinner a few moments previously. Though neither pulse nor respiration could be perceived, nor in a hurried examination could the sounds of the heart be heard (Case 6, series i.), I felt from something about his appearance that he was not dead. An electro-magnetic apparatus was set to work from the spine to the region of the heart; other stimuli were made use of, and after several minutes he slowly recovered. These fits, in a slighter form, recurred almost daily for two years, when he died in one exactly like the first. The heart exhibited fatty degeneration. In some cases this feeling amounts to nothing more than a sense of faintness, a feeling that the person must fall if he does not lay hold of something; and in some instances, as in a case at present under my care, this slight faintness is accompanied by an impression that he is about to die. Such persons do die. In the Table before us, death thus occurred by, as I propose to call it, *syncope lethalis*[*] —fatal syncope—in thirteen cases out of thirty-three in Series i., and in eight out of thirty-five in Series ii. This mode of death is in many cases instantaneous, in other cases death, though sudden, is not so rapid in its occurrence, the fatal faintness is progressive, and death may not occur for several minutes after its onset. (See Case 28, series i.) Facts within my knowledge lead me to think that many distinguished men have thus died, and that in this condition has lain the hidden and frequent cause of many sudden deaths.[†] The paleness of the features, the unaltered state of the pupils, the absence of stertor, and the feeble action of the heart, enable us to distinguish these cases from apoplectic seizures when we see them before death. The age also at which apoplexy occurs most frequently, is not that at which we most frequently find fatty degeneration of the heart. For example, of forty-nine cases of apoplexy, forty-two occurred in persons under sixty, and seven only in those over sixty;[‡] whereas, in seventy-nine cases of fatty degeneration of the heart, forty-one were above sixty years of age.

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[*] See Paulus Ægineta, Syden., Soc.'s edition, vol. i., p. 502; also Lancisi for Apoplexia Syncopalis. De Subit Mort, Romæ, 1707, p. 176.

[†] See several cases of sudden death, the heart in each being flaccid, flabby, and its cavities empty, described by M. T. Chevalier in the Transactions of the Society, vol. i. 2d edition, 1812, p. 156. He called the mode of death *asphyxia idiopathica.*

[‡] See Observations on Cerebral Apoplexy, by the author. London Journal of Medicine, vol. i., 1849.

In the thirteen cases dead by syncope, mentioned in Series I, both sides of the heart were affected in five cases, the left side in seven, the right in one. Syncope, therefore, is connected chiefly with disease of the left side of the heart; in some of these cases the left side was flabby, dilated, and contained much blood. In two cases in which there was hypertrophy of the left ventricle, and the fatty degeneration had but little advanced, I found the ventricle rigidly contracted. It appeared as if the diseased fibres had permitted or excited an irregular action, "a spasm," in fact, "of the heart," and death by syncope from deficient pressure, equally in this case as the other, resulted.

c. Shortness of breathing is recorded as having been present in about one-half the cases in Series. In some cases it appears as a sense of choking or suffication,—the person feels as if breathing through a sponge. In some instances the difficulty of breathing is so slight, that it is scarcely regarded; in others so severe, that the slightest effort, particularly in mounting ascents, is most distressing. A peculiarity was observed in two subjects, one of which still lives, viz., that reading aloud caused no inconvenience, whilst it was distressing to ascend a gentle height.

d. Pain is another amongst the recorded phenomena connected with fatty degeneration of the heart. It was present in twelve cases of Series I, and fourteen of Series II. In eight of the first Series, and in eight of the second, it was confined to the region of the heart. In four of each Series it extended, as in *angina pectoris*, more or less over the chest and down the arm. The pain in some cases immediately preceded death, and was then, in nearly all the cases, found to be connected with rupture of the fibres of the heart. There is some evidence to show that rupture of a few fibres may occasionally occur without being fatal, and cause pain; but in the majority of cases, the pain appears to be due to overdistension of the cavities with blood on the one hand, or to the spasmodic contraction just alluded to on the other, it being remembered, that though the heart in health does not possess common sensibility, it may in diseased conditions acquire this property.

These latter phenomena, that is to say, syncope, breathlessness, and pain may occur separately, or two or all of them may be present at the same time; their combination gives us the characters of the disease known as *syncope anginosa*, or *angina pectoris*. The symptoms or phenomena of this disease were very well recognized,* even before Heberden† or Rougnon,‡ who wrote at the same time, described them as constituting a special disease. Still, from that period to the present, the immediate condition on which they depend has been a source of great doubt. It would lead us from our present object to mention all the opinions on the subject. I shall refer to but a few, and those by some of the chief writers, which appear to be directly connected with the view now presented.

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* Amongst others, see Lancisi, De Subit Mort. 1707, p. 46.

† Medical Transactions; London, vol. iii, 1772.

‡ Lettre à Lorry sur une Maladie Nouvelle; Besançon, 1768.

1. It is said that the phenomena may occur in the absence of any lesion of the heart which can be recognised; such was Heberden's* opinion, and such is that of many other writers. It is very easy to conceive, that fatty degeneration, which has been overlooked so long and by so many, might have been present in these cases. Indeed, Dr Forbes† anticipates some such latent condition, when he says, "It is easy to overlook slight misproportions in the different cavities, and also various morbid conditions of the muscular fibre of the heart, which may have sufficed, nevertheless, to occasion the greatest distress or disorder in the action of the organ."

2. Ossification of the coronary arteries has been regarded by Jenner, Parry, Wall, Black, Burns, Jurine, Kreysig, and others, as one of the most frequent causes of angina pectoris. We have seen the close relation which exists between this condition and fatty degeneration. Different opinions have been entertained as to the mode in which the ossification of these vessels and the symptoms were connected. Some believe that the ossified vessels acted as mere mechanical irritants; others that the nutritive function of the heart and its power were impaired by the state of its blood-vessels. Jenner, Parry, Baillie, and Hodgson entertained this opinion.

3. Fothergill attributed the disease to the simple accumulation of fat.

Lastly, Frank, Jurine, and some others connect it with accumulation of blood in the cavities of the heart and great vessels.

It appears to me, from a review of these opinions, which find a connecting link in the affection of the heart's texture now described, and from the general character of the symptoms, that its phenomena may depend on any cause which can produce irregular distension, or irregular contraction of either of the ventricles, and thus, whilst it tends to disturb the circulation through the lungs, can at the same time lessen the amount of pressure which should exist within the skull. This is not the place to discuss all the causes which may produce these effects. For the present, it is sufficient to show, as has been already done, that this degenerated condition of the heart's fibres is a very sufficient source in itself of the several phenomena, (viz. breathlessness, faintness, and pain,) which are recognised under the name of *angina pectoris* or *syncope anginosa.* Many facts in reference to the age, sex, and habits of life, under which these conditions respectively occur, confirm this opinion. Lastly, in the Tables of the cases, five cases of true angina in connection with this state of heart will be found, viz. Cases 13 and 15, Series I, and Cases 2, 16, and 25, Series II, and I might add a sixth recently communicated to me by Dr Copland. Four other cases, recorded by Fothergill and Parry, will be found in Series III.

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* Commentaries, 4th edition. London, 1816.

† Art. Angina Pectoris; Cyclopædia of Practical Medicine.

With these remarks, I conclude what I have to say on the effects of fatty degeneration of the heart, on the structure and functions of the organ. The influence of *hereditary predisposition* and the *duration of the disease,* are points still open to future investigation. I have found nothing which bears on the former subject in the recorded cases. In one of the cases recorded by myself, a sister had died suddenly in the street, it was said of diseased heart. In another case, the father of a gentleman who is living, but in whose heart I have no doubt the disease exists, died under similar circumstances.*

In regard to the duration of the disease, it is not improbable, that its progress may be rapid in some cases in which the general nutrition is greatly depraved; in other cases, however, in those particularly which occur in advancing life, it is evidently slow in its progress. Thus some cases in the table would seem to show, that it may have existed for two, five, ten, twelve, or fifteen years respectively.

V.—THE DIAGNOSIS OF FATTY DISEASES OF THE HEART.

It would be a refinement at present to seek to distinguish during life between the presence of fatty growth and fatty degeneration; I shall, therefore, allow what I have to say on the diagnosis of fatty degeneration to apply to fatty growth. I have already referred (in a note, p. 143) to the fact, that fatty degeneration of the heart is said to be frequently found after death, where its existence had not been suspected during life, and I have expressed an opinion, that such cases would not often be met with, if a full inquiry could be made into their symptoms. I feel the truth of this observation more particularly in those which occur in the progress of advancing life, when whilst the system generally keeps up its powers tolerably well, the heart suffers from some local cause, such as diseased coronary vessels. In those cases, the balance between the system and the heart is lost, and phenomena, as clear and as pointed as any that can indicate the nature and seat of disease, are developed. Again, on the other hand, in those exhausting diseases in which the heart participates, it is quite possible, that the requirements of the system may not be disproportioned to the powers of the heart, and death may come on slowly and insidiously without our attention being attracted to the state of this organ. In the former class of cases we find, as *symptoms* of the disease, various modifications of the phenomena of drowsiness, coma, faintness, syncope, breathlessness, and pain in the region of the heart, as previously described. A patient complains, in the earliest ages, of being easily exhausted, particularly by mounting heights; he feels, he says, faint, when he gets to the top of the stairs; though not giddy, he feels as if he must fall; and, though not breathless nor panting, sighs deeply, and seeks the air. Any unusual excitement, a heated or a close atmosphere, produces the same effects; at the same time, there is often experienced an uncomfortable feeling of choking, or fulness in the chest. In the intervals, the individual is pretty well. As the disease advances, the attacks become more frequent and severe, and often disturb and distress the patient at night. The temper has been observed to become irritable; in several cases, the expression of the features appears anxious, and frequently the countenance is sallow;[*] œdema of the legs, and copious perspirations from very slight causes appear amongst the associates of the disease. The pulse is generally affected, but the mode in which it is so depends no doubt on the part of the heart affected, and on the extent and degree of the disease. Irregularity is one of its most frequent alterations, weakness is another, slowness a third. They are mentioned respectively, as having been noticed, seven, five, and four times in Series I; and six, nine, and four times in Series II. The pulse is described as being regular only five times in the three series, never as being strong. It is mentioned as being full (three times), once as being hard, and once wiry and small, in a few cases as being quick or easily excited. In general, weakness, irregularity, and slowness[†] of the pulse, are the

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* Since the presentation of this paper, a case of fatty degeneration of the heart has come under my notice, in which the brother and uncle died suddenly of diseased heart. It was said on post-mortem examination, that excess of fat on the heart was the cause of death in one of them.

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[*] It has been recently demonstrated by Mr Canton, that the *arcus senilis* of the cornea is a fatty degeneration of this texture. Dr Williams has observed, that this appearance is very frequently associated with the phenomena here described as characterising the presence of fatty degeneration of the heart; and in one deeply interesting case, at the post-mortem examination of which I assisted, the diseased condition of the heart, to the diagnosis of which the state of the cornea contributed, was most marked. Mr Canton has further informed me, that he has constantly observed the degenerated condition of the heart in the cases of well-marked *arcus senilis*, which he had an opportunity of examining. (See also the Lancet, vol. i. 1850, p. 561.) Thus, then, when the signs and symptoms of fatty degeneration of the heart are present, this appearance of the cornea will greatly aid in the diagnosis. It must not, however, be forgotten that fatty degeneration of the heart may occur under circumstances and at an age when we would not expect to find this lesion of the cornea; so likewise, but in a much less degree, we may expect to find the change in the cornea independently of change in the heart. Dr Williams attributes the arcus to the pressure of the lids on the upper and lower margins of the cornea. The shape and mode of formation confirm this idea.

[†] The slowness of the pulse is sometimes quite remarkable. In one case (No. 6, Series I.), the pulse was as slow as twenty-four in a minute when the patient was lying down, and never above thirty-two. There is an interesting communication by Dr Stokes in the Dublin Journal, August 1846, p. 73, in which he

cters which we most frequently find. The irregularity may
e constant; I have seen it present during the slight attacks
described. I have seen it disappear altogether while the
it was in tolerable health, to return, as the effect of any de-
ng cause, the more marked because that cause may be far
efficient to affect a sound heart. As the disease advances,
mptoms become more marked, the various effects of languid
eble circulation show themselves; *angina pectoris* is per-
ully developed, or the patient is cut off suddenly by some
the effects connected either immediately or remotely with
ion itself. Of the eighty-three cases comprised in the three
, sixty-eight died suddenly. The *physical signs* which cha-
se the disease are few, but very distinct in some instances.
are a feeble impulse of the heart, proportioned to the extent
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ss. M. Rokitansky suggests, that disease of the *columnæ*
may cause imperfection in the action of the valves with
they are connected, and thus give rise to a murmur. In
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This, it appears to me, may depend on the dilatation and
of the ventricle, preventing a perfect action of the semi-
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THE TREATMENT OF FATTY DEGENERATION OF THE HEART.*

have no evidence to show, that we can restore muscular
which have been destroyed; all, therefore, that we can
for in these cases, and that not always, is to arrest or sus-
for a time the progress of the disease by improving the
y of the blood, and thus supporting the vigour of those
ns of the heart still uninjured. The nervous powers being
same time strengthened, and the blood itself rendered a
stimulant. The effect of treatment calculated to promote
objects is often very striking. I have found attention to

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show the connection between slow pulse and disease of the aorta, or its
I am disposed to think that the soft, flabby, feeble, if not fatty, state of
t, which was present in all his cases, is more intimately connected with
of the pulse than the aortic disease, which does not appear to have as-
constant or uniform character in any of the cases.

the treatment of fatty growth on the heart, I have nothing to add to those
principles which may be founded on the causes which produce the disease
ly described. See, for the effects of alkalies, Dict. de Méd. et de Chir.
art. Obesité, by M. Roche; and also Dr Fothergill, on the Effects of
nce, &c., Med. Observ. and Inquiries, vol. v., p. 248, &c., and the article
, by Dr Williams, Cyclopædia of Practical Medicine.

the digestive organs, in the first instance, useful; bitter tonics with alkalies, and subsequently iron in different forms, seemed to act most advantageously. I need not, in a communication already so prolonged, enumerate the habits of life, change of air and scene, dietetic regulations, &c., by which the general health, and with it the part most in need of it, may be improved. In reference to some of the symptoms, I might mention, that I have seen in two or three cases great relief from pain experienced by the repeated application of three or four or more leeches over the region of the heart, followed by a blister. In one case, which I saw with Mr James W. Ilott, of Bromley, some short time ago, attacks of distressing angina ceased for several months, (when the patient died of another disease), after the application of leeches three or four times, and the internal use of iron. Exercise has recently been recommended in the treatment of these affections. The patients cannot take it, if it were desirable that they should do so. If exercise can be taken in the early stages of the disease, it should always be short of producing fatigue. Case 19, Series II., illustrates the ill effects of over exercise, even as a curative agent. Cases 23 and 25 show in a different way the dangerous effects of over-exertion. It may well be borne in mind, that narcotics are given with risk in these cases. One death occurred after an accustomed dose of morphia; another during the inhalation of chloroform. On the other hand, it is needless to indicate the usefulness of antispasmodics in these cases during paroxysmal attacks.

ART. VIII.—*On the Employment of a Long Flexible Stethoscope for the Purpose of the Stethoscopist examining his own Chest.* By THOMAS STRATTON, M.D. (In a Letter to Dr Craigie.)

PRINCE-EDWARD ISLAND, 17*th October* 1851.

MY DEAR SIR,—I had the pleasure of receiving your letter of the 10th of April 1851, acknowledging the receipt of my note of the 20th December 1850, on a Long Flexible Stethoscope. From your letter of 21st August, my note appears to have been mislaid before it reached the printer, and I now beg to send a copy of it.

By employing a long *flexible* stethoscope made of *gutta-percha*, a student may conveniently learn the natural sounds of the heart and respiration, without its being necessary for him to have a second person present, a thing sometimes inconvenient. And students and practitioners may also thus easily attend to any morbid sounds of the chest. For the sounds of the heart, and for the anterior surface of the chest, a stethoscope eighteen inches long suffices; for the posterior surface of the chest, an instrument thirty innces long is requisite. *One* of the greater length will of course answer all purposes. The idea occurred to me on seeing the long *gutta-percha* tubes intended to be used by deaf persons. In the case of infectious diseases, these long stethoscopes would be safer than those of the common length.

I am aware that in my present detached and remote situation (in Prince-Edward Island), in what I propose, I may have been anticipated by others who have practised or published what is here recommended; if such be the case be so good as do not insert this letter.—I am, dear *Sir, yours* sincerely, T. STRATTON.

PART II.

CRITICAL ANALYSIS.

ART. I.—*Traité des Maladies de Plomb ou Saturnines.* Par L. TANQUEREL DES PLANCHES, Docteur de la Faculté de Medecine de Paris. Tome Premier, pp. 550; Tome Second, pp. 551. Paris, 1839. 8vo.

Treatise on the Diseases arising from the Action of Lead on the Human Body. By L. TANQUEREL DES PLANCHES, Doctor of the Faculty of Medicine at Paris. Two volumes, 8vo. Volume i., pp. 550; Volume ii., pp. 551.

2. *Notices of the Effects of Lead upon the System.* By JAMES ALDERSON, M. A., and M. D., Physician to the Hull General Infirmary, Fellow of the Royal College of Physicians, London. Read January 8th, 1839. Transactions of the Royal Medical and Chirurgical Society, volume the twenty-second. London, 1839. 8vo. Pp. 82–94.
3. *On a Remarkable Effect upon the Human Gums, produced by the Absorption of Lead.* By HENRY BURTON, M. D., Fellow of the Royal College of Physicians, and Physician to St Thomas's Hospital. Read January 14th, 1840. Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society, volume the twenty-third. London, 1840. 8vo. Pp. 63.
4. *Cases of Poisoning by Lead at Claremont.* By Dr R. Gneneau de Mussy (Dublin Quarterly Journal. No. 14. May 1849.)
5. *Some Remarks on the contamination of Water by the Poison of Lead; and its Effects on the Human Body.* By JAMES BOWER HARRISON, M. R. C. S. L. London, 1850. 12mo, pp. 32.

THE deleterious effects of lead upon the animal body are so powerful, and act in a manner so insidiously, that it has at all times been a subject of anxious inquiry among members of the medical profession to determine both the exact effects of the mineral, the channels by which it is introduced, and the most efficacious methods of counteracting them. The metal itself is employed very extensively in the arts; and so long as it is difficult or impossible to dispense without employing it, workmen engaged either in smelting it from the ore, or afterwards preparing the metal for the different purposes to which it is applied, are all more or less liable to be affected by its poisonous operation.

Besides those workmen who are engaged in separating the lead from the ores, considerable numbers are employed in converting the metal into the various forms in which it is employed in the arts. The plumber converts it into sheet-lead of various degrees of thickness, for roofing, for lining cisterns, and similar purposes, and into leaden tubes for the conveyance of water in cities and large towns. The type-founder employs it with antimony and other metals for type metal; the house painter and the preparer of pigments employs it very extensively as the colouring matter or the drying agent in his mixtures. The potter employs it in glazing various sorts of earthenware. Various oxydes and salts of lead are employed by paper-stainers, manufacturers of German cards, and the makers of glazed cards. In short, though it has always been a metal extensively employed for different purposes in manufactures and arts, it is probably at present still more extensively used than ever; and the result is, that, notwithstanding all precautions, the number of persons who suffer in different modes from its use is very considerable.

There is reason to believe that the poisonous effects of this metal are much more frequently observed in continental countries than in Great Britain. Since the time at which Sir George Baker pointed out the hurtful consequences of preparing cyder in leaden vessels, and Dr John Hunter showed how often rum, which had been distilled by passing through a leaden worm, contained palpable quantities of this metal, a considerable degree of caution has been observed in this country in the domestic and industrial applications of lead; and the disorders which were understood to arise from the presence of this metal in cyder, perry, and similar articles, have become so much less frequent, that, from this cause, they can scarcely be said to be known as existing. Neither can it be said that diseases believed to proceed from the poisonous effects of lead introduced into the system in other modes, for instance by absorption, by inhalation, by ingestion with food, are very much on the increase.

Very different appears to be the case in France. The present author adduces a long list of trades and professions in which lead diseases are more or less prevalent. It appears indeed from the considerable prevalence of this order of diseases, that M. Tanquerel des Planches has been led to lay before the profession the result of some very elaborate inquiries in which he had been engaged for the space of eight years. In the year 1831 he commenced a series of researches on saturnine or lead diseases; and in February 1834, he published, in his Inaugural Dissertation, the information which he had collected, and the conclusions which he had been enabled to form in one department of the subject, that, namely, of palsy, arising from the poisonous operation of lead. This treatise was looked upon with eyes of favour so encouraging, that the author was induced to prosecute the subject upon a more extensive scale, and with greater amplitude of details and illustration. He then formed the intention of preparing a work on all the diseases or hurtful effects which could be traced to the poisonous operation of lead upon the economy, the different modes in which it was introduced, and the various and variable phenomena to which its presence in the system gave rise.

This enlargement of his plan, however, rendered necessary a course of clinical observations lengthened and laborious. The Hospital de la Charité in Paris is the principal resort of the great number of workmen who are occupied in the different forms of lead manufacture; and in this Hospital M. Tanquerel des Planches found the means of conducting his observations upon an extensive scale and in a systematic manner. The physicians of this large establishment afforded the author every facility in their different departments; and for the space of five years subsequent to 1834,—eight years altogether,—the author was diligently occupied in studying upon the persons of patients the effects produced by this metal, and in endeavouring to discover the most prompt and effectual means of counteracting these effects.

Diseases arising from the poisonous effects of lead are not common unless among poor working people who necessarily apply for assistance at the hospitals. These diseases are accordingly not common in private and civil practice. The author, however, has not neglected this source of information where it was available.

In investigating the channels by which lead in different forms is introduced into the animal body with a view to the establishment of prophylactic measures, M. Tanquerel des Planches has taken frequent occasion to visit the workshops and manufactories at Paris and the neighbourhod; he has followed carefully the processes and operations of each trade; and by carefully conducting his inquiries among the workmen, the overseers, the directors, he has been able to obtain information which he thinks is more accurate and less doubtful than any hitherto adduced.

In adverting to several of the topics which M. Tanquerel des Planches has investigated, our choice must be regulated by their comparative importance, and by the degree in which they have hitherto been imperfectly understood.

When lead reduced to a state of extremely minute division is introduced into the system, the animal economy suffers a deleterious impression more or less intense. This form of poisoning may be made known to the observer by several morbid indications well marked.

It is to be observed that the two divisions of the nervous system which preside, one over the internal or organic life, the other over the life of relation, are equally exposed to become the seat of the different disturbances which constitute Saturnine Poisoning. The mode in which these disturbances take place is not less remarkable. In the nervous system of the internal or organic life is observed only the exaltation or intensification of nervous action. In the nervous system of the life of relation, on the other hand, the phenomena of sensibility and of inability may be sometimes exalted and sometimes extinguished. Thus when lead directs its pernicious influence upon the viscera contained within the abdomen, colic in all its various forms takes place. If the spinal system be the first part attacked, there may appear in the organs of the animal life those acute pains which denote the presence of *arthralgia*, or that loss either in movement or in sensation which distinguish saturnine palsy and anaesthesia. When the brain is affected, delirium, convulsions, or coma, characterize the presence of Saturnine Enkephalopathy. In other respects lead impresses a peculiar stamp on the physiognomy of the diseases to which it gives rise.

Saturnine or lead diseases are not all equally frequent. The numbers presented by the following table, which embraces all the individual examples collected by the author, may be taken to indicate the comparative frequency of each order.

Cases of Colic,	1.217
Arthralgia,	755
Palsy,	127
Enkephalopathy	72

Among these several examples of saturnine diseases, two hundred and seventy-six (276), have not been accompanied with colic. From this statistical return of the proportions, taken in round numbers, neglecting fractions, it appears that among twenty-three individuals affected by saturnine diseases, nearly twelve persons are attacked by colic, eight are the subject of arthralgia, two of palsy, and one person only is the victim of enkephalopathy or disease of the brain.

It is merely another form of this proposition to say that colic is the most frequent form of saturnine poisoning. But very rarely does this disease appear alone; often it is complicated with arthralgia, sometimes with palsy or even enkephalopathy. In other instances each of these diseases appears separately. They may further be combined altogether, or in the number of two or three in the person of the same individual.

Colic, arthralgia, the different sorts of palsy, may equally commence or terminate the process of poisoning.

Though the different forms of saturnine poisoning exert no direct or reciprocal influence upon their production, it must, nevertheless, be allowed, that one lead disease predisposes to others, because it indicates the presence of lead within the system, which then becomes liable to all the disorders proceeding from the action of this metal. Colic, accordingly, being the most common saturnine disease, it is not astonishing that individual labouring under other saturnine affections should often at the same time present symptoms of colic.

The violence or severity of one saturnine disease does not form any indication for the prevalence or intensity of others. It by no means follows that because a patient has sustained a severe attack of colic arthralgia is either to take place at all, or is to be severe, or that palsy or enkephalopathy are more commonly produced than in slight cases.

In surveying persons suffering from lead diseases, it is observed that each time that the peculiar symptoms are produced, the same form of morbid phenomena affect the same individual or order of individuals. Thus one set of persons under the influence of lead poisoning always present symptoms of enkephalopathy, another set always palsy, a third always the predominance of arthralgic symptoms, and a fourth set as uniformly symptoms of colic. These peculiarities indicate the influence of some idiosyncrasy, the nature and the causes of which require investigation.

Closely connected with the circumstance now mentioned is another point deserving remark and inquiry. Among one hundred individuals placed in contact with lead, under circumstances apparently the same, some are attacked by colic, others by arthralgia, some by palsy, others by disease of the brain.

The circumstance now mentioned, the author regards as satisfactorily proving that colic, saturnine arthralgia, palsy, and enkephalopathy are distinct diseases, independent of each other, which may separately indicate the action of lead poisoning, and that the three last are not sympathetic and consecutive effects of colic, the most frequent form of lead poisoning.

The different forms of saturnine disease attack very different organs. The functional lesions which indicate their presence ought accordingly to present to the observer very different morbid aspects. Among the four fundamental saturnine diseases three observe an acute progress, while the course of palsy is generally chronic. It must be also remembered that the treatment of one of these disorders is not suited to the other.

The order in which M. Tanquerel des Planches treats these several disorders is the following: first, Colic; secondly, Saturnine *Arthralgia*; thirdly, Palsy; and fourthly and lastly, *Enkephalopathia*, or Saturnine disease of the Brain.

I. When preparations of lead are introduced into the animal body, previous to the development of the specific diseases, the poisonous operation of the metal is manifested by an action quite specific upon the greater part of the solids and fluids of the system. To this preliminary action M. Tanquerel des Planches gives the name of *primary saturnine intoxication.*

Though all persons exposed to the causes present the marks of this primary toxication, they do not appear with equal speediness in all. For example, the marks of this preliminary poisoning appear very quickly in the great number of workmen engaged in making preparations of lead for purposes in the arts, who are habitually placed amidst an atmosphere of saturnine dust and vapours,—for instance, workmen engaged in smelting lead ores, or manufacturers of white lead, of red lead, of litharge. These persons absorb almost incessantly emanations of lead by the respiratory surfaces and the digestive surfaces, and hence the mineral is both extensively applied and speedily introduced.

Among persons, on the other hand, working amidst an atmosphere not much charged with lead emanations and vapours, and who are seldom surrounded with air much impregnated with lead-vapour,—for instance, house painters, type-founders, glazers of earthenware,—only one or two marks of primary saturnine toxication are recognized. When all the effects of this primary action are evinced in any of this class of workmen, it is only after a long period of time.

We do not observe the effects of the primary action of lead among workmen who handle the metal in the fixed state, and who consequently are never in contact with lead particles disseminated through the air.

1. The first and most frequent phenomenon which indicates the presence of lead in the animal body is a peculiar coloration of the gums and of the teeth.

The portion of the gums nearest to the teeth for a space of one to two lines usually acquires a bluish slate-gray colour. The rest of the gums often exhibits a very slight blue-red aspect, which is insensibly lost in the red tint of the buccal mucous membrane.

In some instances the slate-blue colour attacks the whole extent of the gums and spreads to the buccal membrane. M. Tanquerel des Planches saw in the wards of M. Briquet, at the Cochin Hospital, a white-lead maker, in whom the mucous membrane of the mouth and the tongue was entirely tinged of a slaty-blue; a few red points being perceived only here and there. In some workmen there are seen from space to space patches of blue streaks upon the surface of the buccal mucous membrane. Lastly, in a considerable number of cases the portion of the gums next to the teeth having alone become of a slate-blue colour, presents a decided contrast to the rest of the gums, which preserve their natural rose-red hue.

The gums, before completely acquiring the slaty coloration become at first of a reddish violet tint, which eventually passes after a period variable in length, into blue.

The mucous membrane of the mouth, therefore, may present several degrces and stages of this coloration, and may be consequently stained blue through its whole extent.

This blue colour of the gums cannot be removed without great difficulty. It is only by means of frictions repeated several times in the course of the day for a considerable period, and performed with water acidulated by sulphuric or hydrochloric acid, that it is made to disappear completely.

Often the portion of the gums which becomes blue undergoes a remarkable alteration in nutrition. In some instances it is rendered extremely thin, as much so as the thinnest writing paper, or, what is still more common, it loses part of its surface. In the last case the interdental slips evidently disappear, and the concave part of the gums is removed by a process of molecular absorption, which takes place in the midst of these tissues without apparent solution of continuity.

When this process of absorption is completed the teeth are observed to be stripped of a portion of the gums; they are denuded. The gums then present only a ring, more or less prominent, which sometimes appears as if notched.

This change in the nutrition of the gums always follows the formation of the blue stain. But the latter often disappears at the same rate at which the interstitial absorption carries away the blue-stained portions of the gums. The ring also formed in this manner scarcely presents a bluish shade.

In ordinary circumstances the blue stain and the loss of substance in the gums are not observed to be equally well marked along the two alveolar margins. The anterior part of the jaws is the point at which the twofold alteration in general is most distinctly marked. The gums of the lower jaw are uniformly a little more affected than those of the upper jaw.

In some instances it happens that two or three gums only become blue coloured and undergo this loss of substance.

In a very small number of workmen M. Tanquerel des Planches observed that the gums stained blue were congested, and bled upon the slightest touch.

In one instance he found in the case of a white-lead maker an ulceration of the superior margin of the gums of both jaws. In this man the mucous membrane of the cheeks was almost everywhere stained blue. He did not suffer uneasiness. Lotions of water acidulated with sulphuric acid, which he employed in order to remove the blue staining, contributed also to heal the ulceration.

Most usually, among persons whose gums in contact with lead have acquired a slate-coloured stain, the teeth present at their base or collar, a brownish tint, very deep; while their gums present most frequently an aspect of a clearer brown, bordering upon yellow or green. This colour cannot be confounded with the ordinary tartar which covers the teeth. It is the incisors and the canine teeth which are mostly found coloured brown; but all the teeth may be so, though not all in the same degree. Where the gums have been removed by absorption, that is, by loss of substance, the brown coloration of the collar is not so well marked.

The teeth thus stained eventually become weak, brittle, are cracked, carious, and drop out before the ordinary time.

This colouring matter adheres closely to the bony substance of the teeth, with which it appears to be intimately united, and, like that of the gums, it is removed with great difficulty. It may, however, disappear under the use of the same means.

In general, when the blue stain of the gums is strongly marked, the brownish colouring of the teeth is equally distinct; and the converse.

This colouring matter of the gums and the teeth is nothing but sulphuret of lead. Oxygenated water placed in contact with the gums and the teeth so stained gives rise to a whitish streak, which is sulphate of lead. M. Tanquerel des Planches digested in hydro-sulphuric acid the gums and teeth of a colour-grinder, who had died in consequence of saturnine disease in the brain; (*enkephalopathia*). At the end of twenty-four hours, a great part of the gums, which was not visibly covered by this bluish staining, became of a deep blue colour.

Of the mode in which this substance is formed on the gums and teeth of persons working among lead preparations, the author gives the following explanation. Alimentary articles comminuted by mastication leave some of their fragments in the mouth or interposed between the teeth. Amidst these remains of alimentary articles, which always contain a little sulphur, and are speedily decomposed, sulphuretted hydrogen gas is formed. This gas and the minute particles of lead which pass through the mouths of individuals who respire or swallow such particles, are placed in mutual contact; and in consequence of chemical affinities, these substances are decomposed, and sulphuret of lead is consequently formed, and deposited upon the gums and the teeth in those very spots where the sulphuretted hydrogen is produced.

If in other parts of the economy besides the mouth, sulphuretted hydrogen is formed in the uncombined state, undoubtedly we should ascertain in these parts the presence of sulphuret of lead in instances of saturnine impregnation; that is, when the lead particles would come into contact with this gas. In the whole organized body sulphur enters as an element of several of the solids and liquids; but it is intimately united with other constituent substances of animal matter; and this union forms an obstacle to disaggregation, and consequently to its conversion into free sulphuretted hydrogen capable of being combined with the lead.

Saliva, for example, in the composition of which there is a considerable amount of sulphur, is not decomposed in the presence of lead. Its sulphur does not combine with the particles of this metal in sufficient number to hold the lead in solution or simply in mixture, in persons in the early stage of lead impregnation. M. Tanquerel des Planches examined this saliva by the microscope, and also chemically. Nothing abnormal was found.

The loss of substance in the gums by interstitial absorption or *molecular* atrophy, their congestion and ulceration, as well as caries and destruction of the teeth, are probably explained by the obstruction of the capillary vessels arising from the accumulation of sulphuret of lead. This renders nutrition impossible.

After from five to six days, months, or whole years of exposure to the contact of lead particles, we observe the brown staining of the teeth and the slaty staining of the mucous membrane of the mouth to be established.

A great number of white lead makers present marks of this staining at the end of some days of working; house-painters are usually attacked only at the end of some months of the exercise of their trade; while among crystal cutters and polishers the indications of this formation usually do not take place until after several years of working.

The colour of the gums and teeth now described M. Tanquerel des Planches never observed except among individuals in whom the mucous membrane of the mouth is in contact with particles of lead. He examined the mouth in seven hundred and eighty-five persons, who had not worked among lead preparations, or who had not swallowed the metal in any manner; in not one of these did he discover the slightest trace of the specific staining produced by sulphuret of lead.

This stain, therefore, he infers, forms an excellent means for recognizing whether any individual has swallowed or respired particles of lead. It is in truth a diagnostic mark of the primary action of lead upon the economy.

Nor in all persons who respire or swallow particles of lead for some time, are the teeth and gums covered with sulphuret of lead; but the author met only with a very small number of these in whom this coloration was not perceived.

2. *Saturnine taste, breath, and odour.*—The workmen on the gums and teeth of whom there is a considerable deposit of sulphuret of lead complain often of a peculiar taste in the mouth. Some, and this is the most frequent case, represent it to be a saccharine, styptic, astringent taste, quite similar to that which might be produced by a saturnine preparation deposited upon the mucous membrane of the mouth. Other workmen compare this taste to something at once fetid and styptic.

Saliva is not habitually increased in amount. Some persons say that their mouth is dry; it would appear that the amount of the secretion is less than in the normal state.

Of the breath the odour is often quite characteristic. The sensation of fetor which it causes the observer to experience alone can convey the idea of this odour, which otherwise cannot be described. M. Tanquerel des Planches calls it the saturnine breath. If once felt, he states, the odour cannot be forgotten. Of this odour some workmen are conscious.

When we remain for some minutes or some hours in workshops where particles of lead are disseminated in great quantity in the atmosphere, we are sensible of a peculiar odour, which is that of the saturnine substance affecting by its emanation the organ of smell. The author observes, that it is sufficient to rub for some minutes with the fingers a bit of metallic lead, in order that the organ of smell may be affected with a peculiar sensation, by the contact of saturnine emanations, which are detached during this friction. It may be added, that in all situations where lead is melted, or even heated, the same odour is perceptible.

3. *Lead jaundice.*—This not very correct denomination is applied to a peculiar dull dirty yellow or ashy colour of the skin when the system is impregnated with the metal in a state of minute division. Though it is most marked upon the face, yet the skin of the trunk and the extremities may equally assume this peculiar colour, though in a slighter degree. Upon the conjunctiva, this dirty yellow colour is well marked, being also there associated with a distinct bluish staining of this membrane.

The colour of the urine inclines to the yellow, from, it is said, the same impregnation. Nitric acid produces not the play of colours represented by Berzelius to be the proof of the presence of the colouring matter of the bile.

The fecal matters present a tawny yellow, which is said to depend on the same cause.

The serum of the blood presents a faint yellow reflection; but this yellowish tint does not incline to green.

M. Tanquerel des Planches found this peculiar earthy yellow tint after death in almost all the organs of the system; in the brain, lungs, heart, intestines, stomach, liver, kidneys, and bladder.

This saturnine colouring of the surface is in some instances rapidly formed in consequence of exposure to lead emanations. White lead makers often become yellow at the end of from ten to twenty-five days of working. Among others, however, who handle lead preparations without absorbing them, a longer time elapses before the surface acquires a yellow colour. Such is the case with house painters, makers of German cards, and similar artisans. Lastly, many, especially workmen in manufactories of chromate of lead, metal varnishers, or japanners, and others, are not affected by it.

M. Tanquerel des Planches regards this condition as the result of absorption of minute particles of lead into the animal body, its being extensively mixed with the blood, and in this manner affecting the surface as well as the deep-seated parts.

It appears, nevertheless, that the presence of lead has not yet been chemically discovered either in the blood or in the organs, in instances of saturnine jaundice and saturnine diseases. It is possible that this may depend upon the imperfection of the means of research. If not, the inference must be, that the lead produces the peculiar colour of the tissues by means of some specific action while it passes through the system.

This colour the author has never seen except in persons whose respiratory and digestive organs have absorbed particles of lead. If persons so coloured quit the places and occupations in which they have contracted the colour, it gradually disappears, and does not return.

The colour is said to be so characteristic that it is impossible to confound it with common jaundice; and by it alone patients suffering under saturnine diseases may be recognized among others.

4. With the symptoms now mentioned is very uniformly associated a certain degree of wasting and emaciation. This is general, but is most marked in the face. The fat disappears, the muscles are not unfrequently diminished in size, and in the face particularly furrows and wrinkles give an expression of melancholy to the countenance.

The time at which this wasting may ensue to a degree great enough to attract notice varies from fifteen days or one month to years. All depends upon the degree in which the individuals are exposed to the emanations and absorption of the lead particles.

It appears also to bear a proportion pretty exact to the amount of the metal absorbed. Workmen may be subjected to hard and excessive labour and poor diet; but these circumstances are not sufficient to account for this wasting away of the textures. It operates directly in impairing nutrition; and the sufferers invariably complain of corresponding want of strength.

5. According to the observations of the author the circulation and its movements are considerably affected. In general, the cardiac and the arterial beats are more feeble than in healthy persons; the arterial pulse is always smaller. The contractions also diminish in number,—falling to 55, 50, 45, 40, in the minute in persons, whose pulse previously beat from 60 to 70 times in the minute.

Of all these phenomena, the most important and that which is most speedily formed is the blue stain of the gums and the brownish colouring of the teeth. In many workmen who handle lead and its preparations, most commonly this mark of the primary action of the metal is the only one observed. Such are those whose systems absorb only a small portion of the metal, or little at a time; for instance, tinsmiths, type-founders, earthenware glaziers and similar trades. In numbers of other trades, as manufacturers of chromate of lead, metal-varnishers, and persons who use internally lead preparations as medicine, the peculiar tint is more rarely observed.

Since the foregoing account of this peculiar staining of the gums and teeth was made known by M. Tanquerel des Planches, a description of the phenomenon has been given by the late Dr Henry Burton of London. The attention of Dr Burton was first directed to the appearance in the year 1834, in consequence of a patient under the treatment of Dr Roots at St Thomas' Hospital having been said to be salivated by the internal use of acetate of lead. From that time Dr Burton was accustomed to examine the mouths of patients admitted into his wards, who had been exposed to the action of lead in the course of their usual occupation, and also of those who had taken the acetate of lead medicinally. This investigation led to the belief that salivation, in the ordinary sense of the word, does not occur in one case among thirty-six instances of lead colic; nor in one case among fourteen instances of pulmonary disease treated by means of acetate of lead. But in the total number of patients examined while under the influence of lead, there was observed on the gums a peculiar discoloration, which could not be discovered in the gums of several hundred patients who were not under the influence of this metal. As Dr Burton believed that this discoloration could not be produced by any other internal remedy, he naturally inferred that it forms a serviceable diagnostic mark for indicating the presence of lead in the system.

The following sketch by Dr Burton may be usefully compared with the account given by M. Tanquerel des Planches.

"The discoloration was carefully observed on fifty patients, and

ugh it varied a little in point of intensity as well as extent, yet
ollowing description will apply with sufficient accuracy to the
rity of cases in which it was remarked. The other phenomena
rible to the state of the mouth, noticed on these patients, were
er peculiar, nor invariably present. The edges of the gums
hed to the necks of two or more teeth of either jaw, were dis-
ly bordered by a narrow leaden-blue line, about the one-twentieth
of an inch in width, whilst the substance of the gum apparently
ned its ordinary colour and condition, so far as could be deter-
d by comparing the gums of these patients with those of other
nts of the same class in the hospital: there was no invariable
faction, softening, or tenderness about them; neither was there
eculiar fœtor in the breath, nor increased salivary discharge to
served on any of the fifty patients; and on thirteen out of
een patients, who were treated in the hospital with acetate of
and carefully watched during its employment, the substance of
ums, the smell of the breath, as well as the quantity and colour
e saliva, preserved the same characters, after the appearance of
he line, as they respectively possessed before the saturnine pre-
ion was administered; but on the fourteenth patient, who died
hæmoptysis, the gums, which were previously to the use of
tumid and soft, became contracted and firm, after the blue line
ppeared.

With reference to the state induced by lead, it should be re-
bered, on making an examination of the mouth, that the gums
reath of patients who frequent hospitals (and by whom the
ice of cleansing the teeth is habitually neglected), very often
nt an unhealthy aspect, independent of constitutional disease
g from lead; and amongst the fifty patients who were ex-
ed under the influence of this metal, as well as others not under
ifluence, the gums of many were either ulcerated, tumid, or
lly detached from the teeth by incrustations; but even on the
nts with ulcerated gums, the peculiar leaden-blue border line
listinctly visible. I do not remember to have seen one example
e bleeding tumified gum, peculiar to confirmed scorbutus, pro-
l by the internal use of lead; nor do I think it consistent with
rience or reasonable, to suppose that a powerful and very useful
gent in hæmorrhages should simultaneously check hæmoptysis,
roduce bleeding and tumid gums. The colour also of the scor-
gum differs from the blue colour produced by lead, and there
ewise a peculiar fœtor in the breath of scorbutic patients with
ing gums, which did not exist in the fifty patients above al-
to.

either is the state of the gums and salivary glands induced by
rial preparations, similar to that produced by those of lead;
fourteen cases of pulmonary disease treated with the acetate
d, no pain, heat, redness, or tumefaction of the gums, charac-
ic of the action of mercurials, were observed; nor was there
creased flow of saliva, nor looseness of the teeth, notwithstand-

ing the blue line was evident on the gums of all the fourteen patients. On the contrary, the blue line was obliterated on some patients with lead colic, to whom calomel was administered in quantity sufficient to affect the system.

"I am bound, however, in fairness to state that 'turgidity of the gums' is said by Dr A. T. Thomson to supervene large doses of acetate of lead; and also that my friend Mr Moyle of Chacewater has attributed to the action of the same salt, a slight enlargement of the submaxillary glands and a tenderness in them when submitted to pressure; but in no case could he detect any increased salivary secretion; and he further remarks that in one case the gums were firm and dry, and rather pale excepting at their edges, where he observed the blue line. I do not wish to assert that salivation and turgidity of the gums are never produced by the internal operation of lead; but I venture to affirm they are rare occurrences, and not characteristic of its influence. On the other hand, the discoloration is a very constant occurrence; it precedes all other unequivocal symptoms produced by lead, and is not equally exposed to the imputation urged against most medical data, of being fugitive and deceptive. For the discoloration is very permanent; it has endured through months and until death, and having been once observed may be afterwards easily recognised. On a few patients only had it entirely disappeared before they quitted the hospital; on others it had only partially vanished. In many it continued with little or no change; and on a few patients who died after the medicinal use of lead had affected their gums, the discoloration appeared more distinct a few hours after death, and before putrefaction could have began, than during life. It cannot be confounded, when distinct, with the ordinary colour of the gums, during life; and after death, any ambiguity which might have existed previously will be entirely removed by the strong contrast of colours disclosed on the gums of the dead body. The pathognomic value of the discoloration will bear a proportion to the regularity of its appearance under similar conditions; and in some cases, a little ambiguity may arise from the difficulty of discriminating between imperfectly defined colours; but this ambiguity will soon cease if the patient continues exposed to the action of fresh portions of lead; and in all cases the phenomenon will possess some importance if viewed in connexion with the ordinary symptoms of the presence of lead.

"I have frequently experienced the utility of examining the gums, and in confirmation of my assertion I may add, that the gums of fifty-four patients, men, women, and children, were examined purposely on the same day, to detect this discoloration; but on fifty-two, nothing remarkable was noticed; on the remaining two patients, however, the peculiar blue line was unexpectedly observed, and served in both cases to establish a precise diagnosis.

"One of these patients had worked in a white lead manufactory three months previous to his admission into the hospital; the other in a glass-house; and on both there was a slight paralysis of the wrists, which had been overlooked by myself, and disregarded by the patients themselves at their first examination.

"Two other patients, on whom the discoloration was extremely well developed, were subsequently treated by me, but they were unable to give any satisfactory account of having ever been exposed to the action of lead, although they exhibited the common symptoms which are generally attributed to the influence of this metal, as well as the blue line; and both cases deserve further notice in illustration of its therapeutic utility.

"The first of these two latter patients was a carpenter. He had never worked in lead, nor had he any suspicion of having been exposed to its influence; but he had experienced a severe illness about four years before his admission into the hospital, which had been followed by a partial paralysis of the fingers of his left hand. In other respects his health was restored, and continued good until a few weeks before he was placed under my care; he then began to feel languid, and to experience a sense of weight about the limbs; his appetite failed, and subsequently he suffered a pain in the stomach, which extended upwards over both breasts to the shoulders and down the arms; the bowels had been constipated for a week previous to his admission, and during this interval vomiting had several times occurred. His nights had been passed without sleep; his pulse was 96, soft and regular, his skin warm, his countenance pale. In addition to these symptoms tremors were noticed in both hands when the patient extended his arms, and the gums were very distinctly marked with a leaden-blue border line. The combination of symptoms in this case was such as indicates lead colic and paralysis of the wrists; but in what manner lead was introduced into the system could not be ascertained.

"The second patient was a cordwainer, who had, until his admission, resided in the country. The features of this man were sallow; he was spare, entirely free from paralysis of the voluntary muscles, but he had experienced for several years, at intervals, repeated attacks of colic, by which he had been confined to his bed seventeen times. During these attacks he had endured violent pains in the abdomen, frequent vomiting, and obstinate constipation, sleepless nights and loss of appetite. The gums of this patient were rather turgid, although not more so than nine-tenths of the gums of those patients who resort to hospitals; they were also very well marked with the peculiar blue line, but no other evidence of the patient having been exposed to the action of lead could be obtained. I believe, however, notwithstanding the absence of demonstrative proof, that this patient, as well as the former, was under the deleterious influence of lead. I admit, nevertheless, that reasonable objections may be raised against the validity of the evidence on which this opinion rests; but when the assemblage of symptoms noticed on both patients is compared with that which can be produced by the internal use of acetate of lead, the evidence is almost as conclusive as the nature of medical investigations will admit; for although neither of these patients could give a cause for his illness, yet the supposition that both of them were under the influence of lead is quite consistent with the facts related in the numerous histories published of its action on man, and which, in the cases of the two men under consideration, suggest the extreme probability that they had been unwittingly exposed to the action of the metal, derived from one or more of those many sources which have been elaborately pointed out by Sir George Baker, and subsequently by Dr Christison."—Pp. 66–72.

From various facts recorded by different authors, and from personal experience, Dr Burton expresses strongly the belief that the introduction of lead into the human body is continually taking place, in an unobserved manner, to a much greater extent than is usually supposed, and that it has often caused an ambiguous assemblage of morbid symptoms. This is very likely to be the case; and the fact was well illustrated in the instance of the ex-royal family of France at Claremont in 1848.

From all the facts, in short, which had come under his notice, Dr Burton was led to form an inference very similar to those given by M. Tanquerel des Planches, namely, that the blue line upon the gums may serve not only as an excellent and sure diagnostic mark of the presence of lead in the system, but that, as this takes place in general previous to the formation of the symptoms constituting lead colic, it may be taken as a means to warn the physician in administering lead, when he should stop the exhibition of the remedy. In reference to this subject, among twenty-seven patients treated by Dr Burton during the course of a few years, by means of acetate of lead and opium, in at least twenty persons no colic symptoms and no material inconvenience were induced except constipation. In two or three cases the colic symptoms were severe; but in these hemorrhage was profuse, and the dose was large. With ordinary precautions, however, colic does not occur in a severe form during the administration of lead.

From a table of eleven instances given by Dr Burton, it appears that the shortest time for the production of the blue line upon the gums is four days; and that the smallest quantity under which it was produced was fifteen grains of sugar of lead. In two cases it took seven days; in the others it took ten, eleven, twelve, fourteen, and twenty-one days. In the case last mentioned, 160 grains of sugar of lead were taken to produce the blue line; the patient had no colic; but death took place in consequence of hemorrhage from the lungs. Dr Burton further states that Mr Moyle of Chacewater produced the discoloration in the course of twenty-four hours, by giving four doses of five grains each every six hours.

The question has been stated more than once, whether the credit of discovering this blue stain as an effect of the primary action of lead, belong to Dr Burton. There is no doubt that it was first described in 1839 by M. Tanquerel des Planches. But the late Dr Burton appears to have discovered the fact of the existence of this line, altogether independently, probably so early as 1834; and while he has decidedly the merit of making it first known in England in 1840, it would be ungracious to regard him in any other light than that of a discoverer.

II. After the system has been duly charged with lead, various morbid effects besides the preliminary ones appear. The most common, it has been already said, is that denominated Colic. We here, however, at the first outset encounter certain difficulties, which have tended to throw confusion and some degree of uncertainty on the name of this disorder and its alleged origin. It has not been denied that when lead is introduced into the system, it gives rise to symptoms of severe colic with obstruction of the bowels. But, on the other hand, it has been said with some pretext of reason, that symptoms which closely resemble lead colic may arise in the human body in circumstances, in which lead has not been introduced. There are districts in England, France, Spain, and the West Indies, in which a disease not to be distinguished from painter's colic, or the primary effects of lead upon the intestinal tube, prevails during certain seasons to such extent as to be accounted endemial. This is the Devonshire colic, or Bellain of England, the *Colica Pictonum*, or colic of Poitou in France, the *Entripado* of Spain, and the dry Belly-ache of the West Indies. Though various physicians have represented these forms of disorder to be dependent on the poison of lead introduced in various modes and under different forms into the human body, yet others have maintained, apparently not without good grounds, that no satisfactory proof of the fact ever has been adduced; and they have, on the other hand, considered the symptoms to be connected in some measure with the climate, with the habits of the inhabitants, with the food upon which they subsist. One of the most decided of the opponents of the saturnine origin of the dry belly-ache of the West Indies, and certainly not the least ingenious, is Dr Musgrave of the Island of Antigua, who in 1826 published in the Medical Repository an instructive memoir upon that disease under the name of *Hepatic Ileus.* Dr Musgrave regards the dry belly-ache of the West Indies as entirely a miasmatic or telluric and in some degree a climatic disease.

Citois, who described the disease as he witnessed it at Poitou in the sixteenth century, applied to the assemblage of symptoms which he was in the habit of seeing the name of Colic of Poitou; and as the district was supposed to be the seat of the ancient

Celtic tribe of Pictones, the disease was called in the language of that time *Colica Pictonum.* It was unfortunate that this name was so nearly allied to the Latin term for painters; for in consequence of these persons occasionally presenting the same or a similar disease, it was called *Colica Pictorum.* The names were sufficiently distinct, both in origin and designation, as well as in sound and quantity; but by careless persons they were often confounded, and it appears that still they are occasionally confounded.

Both Citois in France, and Le Pois, Sennert, and Wepfer in various parts of Germany and Switzerland, had seen a form of colic which they believed depended on the use of acid eager wines and other liquors prepared from unripe fruits, and sometimes from cyder badly fermented.. Sauvages, who was well acquainted with the labours of preceding and contemporary physicians, referred all the forms of both species of disease to the head of *Rachialgia,* a name proposed by Astruc, who believed the chief seat of the painful sufferings to be in the spinal marrow and the emergent or ingredient nerves. Sauvages says it must be allowed that, though the theory of Astruc was not confirmed, yet the name was suitable, and therefore ought to be retained.

Sauvages, in arranging under this head of *Rachialgia* or Spine-ache, all the forms of colic, whether originating from lead poison or from other causes, regarded the endemial colic or that in which the operation of lead had not been proved, as a colic of vegetable nature and origin, that is, as proceeding from the use of unripe and acid fruits and liquors thence manufactured. This distinction, which is in some measure necessary, and solves as it were the theory M. Tanquerel des Planches adopts. He admits, in short, that there may exist a disease in all respects similar to lead colic yet not proceeding from the action of that metal. But, on the other hand, he proceeds upon the idea that lead colic is an actual existence, and is a very prevalent result of lead poisoning.

In speaking of the names by which the disease is known in different countries, he falls into a natural and very pardonable mistake, in stating that lead colic is known among the English under the name of Mill-reek. It is not, to the best of our knowledge, in England that this name is employed. It is confined to the Leadhill and Wanlockhead district in Scotland. If anywhere employed in England it is in the northern counties, as Northumberland and Cumberland; but this is mentioned only doubtfully.

M. Tanquerel des Planches gives a view of the literature of this subject, which is chiefly valuable after the time of Sir George Baker, who had written with so much learning and judgment in the Medical Transactions between 1767 and 1785, that nothing deserving to be known had escaped his notice. Upon this part of the subject, therefore, it is quite unnecessary to enter.

The consideration of the modes in which the poison may be introduced into the animal body is a matter both of interest and practical importance.

Saturnine preparations, before entering the system so as to affect the actions of the body, must undergo a previous solution in a liquid, or must be reduced to a state of very minute division, or must be presented in the form of vapour.

All saturnine preparations, metallic lead is of course excepted, are more or less soluble in a great number of fluids of the mineral, vegetable, and animal kingdoms. They are easily reduced into fine dust or powder, which, by reason of its lightness, may be disseminated in the atmosphere, particularly by the aid of currents of air. The saturnine chemical compounds, that is the oxides and salts, are not of themselves very susceptible of being converted into vapour in the open air. Metallic lead appears to be the only article capable of undergoing in a slight degree this transformation. But the former may be volatilized, that is, they may be detached in the form of a thin vapour when they are exposed in certain circumstances to currents of gas or of vapour. In certain circumstances the dispersion of saturnine particles is effected by the aid of a volatile vehicle,—not by themselves.

The author allows that the terms *volatile* and *volatilized* are not in this instance very accurately employed. All that is meant is that the saturnine particles are put in that mechanical condition in which they are carried by the atmosphere or by similar vapours to the surfaces of the animal body.

The saturnine particles, whether solid or gaseous, in contact with the membranes of absorption, must, as all other substances foreign to lead, undergo previous solution by the organic liquids in order to be absorbed. It is only in consequence of this absorption that particles of lead can give rise to saturnine colic; and this circumstance it is, which forms a decided difference between this disease and the accident which constitutes inflammatory poisoning by the introduction of poisons of lead into the stomach. The former operates through an indirect, the latter through a direct action. The former operates by minute quantities gradually and successively accumulated ; the latter produces its effect at once by the largeness of its amount and the speediness and energy of its action. In the last case the poison is not absorbed, and does not require to be absorbed. It irritates the mucous membrane in contact with which it is placed, and may cause in it inflammation and destroy it. This accident, consequently, which belongs to the head of poisoning by irritant or corrosive substances, requires not to be considered in this place.

Lead and all its compounds appear to be capable of giving rise to lead colic. At least all the saturnine preparations which hitherto have been placed in contact with the organs of the human body are endowed with this faculty.

The following list of these substances is given.

METALLIC LEAD.

OXIDES OF LEAD.

Suboxide; (the oxide of Berzelius). The *pulverulent protoxide;* (massicot). The melted *protoxide*; (Litharge). *Protoxide* and *Binoxide*; (minium).

SALTS OF LEAD.

Borate of Lead.

Subcarbonate of Lead (Ceruse; White Lead; White Silver; White of Krems; Venice White; Hamburgh White); (Dutch White), *Neutral carbonate of Lead.*

Phosphate of Lead.

Chromate of Lead; (Mineral Yellow; Paris Yellow; Verona Yellow; Turner's Yellow; Kassler's Yellow; Chrome Yellow; Siberian Red Lead).

Azotate of Lead; (Nitrate of Lead).

Acetate of Lead; (Neutral Acetate of Lead).

Sal Saturni; (Sugar of Lead).

Subacetate of Lead; (*Extractum Saturni;* vegeto-mineral water; extract of Goulard).

Hydrocyanate of Lead; (Cyanuret of Lead).

Silicate of Lead.

Sulphuret of Lead (Black Lead; not the carburet of iron).

Sulphurous Lead Ore; (Alquifoux).

Chloride of Lead; (horn-lead; hydrochlorate of lead).

ALLOYS OF LEAD.

Alloys of Lead and Tin; (Plumber's Solder).

Alloy of Lead and Antimony; of *Lead and Copper;* of *Lead and Silver;* (argentiferous lead ore); *Alloy of Lead and Gold.*

Other saturnine compounds, which are puce-coloured oxide, the molybdate of lead, iodate of lead, seleniuret of lead, arseniuret of lead, alloy of lead with iron, plumbites of potass, of soda, of baryta, of strontia, and of lime, oxalate of lead,—being only products artificially procured and without use, the human race are not often placed in circumstances such that they can absorb these articles, and consequently are not attacked by colic from their introduction into the system. If, however, these substances were extensively distributed in the natural world, or worked and manufactured for the purposes of social life, it cannot be doubted that they are capable of giving rise to this disease.

Some writers have denied that metallic lead possesses the property of engendering the lead colic. This assertion is contradicted by many facts derived from the trades of polishers of printing types, lapidaries, glass grinders. A very good example was given in 1772 by Sir George Baker in the second volume of the Medical Transactions, from the person who sat working at the leaden wheel in a glass-polishing manufactory.* He is described as a pale, emaciated, paralytic person, while all the other workmen seemed to be in excellent health.

It appears, nevertheless, that metallic lead is not very often an antecedent of the saturnine colic. The reason assigned by M. Tanquerel is, that in the different operations in which metallic lead is employed it is much less frequently than its compounds reduced into vapour and impalpable powder capable of dissemination through the atmosphere,—a condition both favourable and in some sense indispensable to the production of saturnine colic.

Though the author thinks it must be admitted, that lead penetrates in the metallic state into the economy, he nevertheless allows that it is possible, that, before being absorbed by the capillary vessels, it may have been at the mucous surfaces transformed into an oxide, a salt, in short, into a soluble body. This seems most probable, especially when it is remembered that at most of the mucous surfaces there are liquids either of acid or alkaline properties; and that in the pulmonary or bronchial membrane itself the oxygen of the inspired air, and the carbonic acid of the expired air, may with little difficulty act on the lead in minute division.

In tracing the channels by which lead enters into the human body, difficulties attend the inquiry whether it is received by cutaneous absorption. It appears from the researches of the author that in ordinary circumstances the epidermis forms a considerable and an effectual barrier to its admission. Thus he found that neither the continued and general use of Goulard's extract, or of the diachylon plaster, or the diapalmic plaster, or the Mother's ointment, as it is named, was followed by the production of lead colic, where the epidermis was entire. It appears also that the members and performers at the different theatres, all of whom use more or less extensively pigments made from lead, never suffer any inconvenience in consequence of this practice. If, in short, lead is absorbed under these circumstances, which it is difficult to prove, it must be in a proportion too small to give rise to the production of any of the saturnine diseases.

M. Tanquerel further endeavoured to ascertain experimentally whether a salt of lead rubbed over the skin with the epidermis entire could produce in animals the symptoms of saturnine poisoning. With this view he rubbed over the skin of dogs strong ceruse ointment; he applied the diapalmic plaster secured by diachylon plaster, and the Mother ointment over the shaved skin; on a rabbit he applied strong litharge ointment, all in such a manner as to show by chemical tests that the lead was well applied. In no instance, nevertheless, were any symptoms of saturnine poisoning produced.

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* Medical Transactions of the Royal College of Physicians, vol. ii., London, 1772. Further Observations on the Poison of Lead, by George Baker, M.D., &c., vol. xx., p. 455.

M. Tanquerel accordingly concludes that the statements made by many authors regarding the cutaneous absorption of lead, while the epidermis is entire, cannot be credited. The epidermis in truth forms an obstacle either quite unsurmountable or not easily penetrable by any of the common salts of lead.

It must not be forgotten, nevertheless, that Orfila, Dr. Pereira, and other good authorities, regard as inconclusive these arguments regarding the non-absorption of lead through the skin, while the epidermis is entire. Dr Alfred Taylor,* who refers to various known instances of lead poisoning by external application, speaks of it as a point not to be questioned. He states, that even the pure metal frequently handled may thus find its way into the system, unless strict cleanliness be observed. This we understand to apply to the condition of the epidermis being retained in a state of integrity. The statement made by M. Tanquerel, that actors who employ carbonate of lead as a pigment, to give the pale colour to the countenance, are not attacked by colic, is contradicted by other testimony. But independent of this, it seems difficult, without admitting cutaneous and epidermal absorption, to account for the bad effects which, wearing Brussel's lace, occasionally produces upon some persons. This manufacture is whitened by means of carbonate of lead; and serious bad symptoms are said to have, in certain instances, followed in those who wear it. In short, at present it must be observed, that the inferences of M. Tanquerel, regarding the absolute protecting and resisting power of the epidermis, must be received with limitation.

On the other hand, when the epidermis is removed, and consequently in open sores and excoriated surfaces, the absorption of lead is known frequently to take place, though not always to such extent as to produce the peculiar symptoms of lead colic.

By the mucous surfaces absorption is produced with little difficulty. Indications of such absorption have been observed to take place by the ophthalmic mucous membrane; by the vaginal mucous membrane, and the extensive membrane of the alimentary canal.

Medicines prepared from lead, especially various preparations of sugar of lead, are, if not the most usual and direct modes by which lead is introduced, at least not uncommon means. M. Tanquerel, after referring to various recorded examples of this incident, expresses the opinion, that saturnine preparations introduced as medicines occasion colic at present, more frequently than is usually believed. He states that he has seen these substances administered only four or five times, and twice has he seen colic and other saturnine diseases induced. He thinks that when intestinal symptoms take place under the use of these agents, they are either overlooked or are ascribed to the original disease, This, we think, is not very likely in this country, But at all events it may be not improper to refer M. Tanquerel to the memoir of the late Dr Burton upon this point.

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* Taylor's Toxicology, p. 448.

Lead is liable to be introduced into the alimentary canal in articles of food and drink. Gaubius affirms that in Belgium some unprincipled dealers mingle white-lead with the butter, in order to increase the weight of the butter, and to give it a white colour, which it appears in that country and at that time was highly prized. In consequence of this adulteration the physician saw several instances of lead colic produced.

Most kitchen utensils and vessels, especially those of copper, are lined by an alloy or tinning, which consists of tin in large quantity and a small proportion of lead. When this tinning is new and well done it causes no inconvenience. But when it is partly worn, in consequence of long-continued use, some portions of this alloy may be mixed with the food prepared in these vessels, and may thus become the cause of severe accidents. Thus it is reported by Barruel, that a pudding baked in a copper pan or shape, the tinning of which was worn, produced severe symptoms of abdominal poisoning, owing to some particles of oxide of lead which it contained, and which were derived from this tinning.* The lead probably did mischief; but it may be asked, in this case, what was the state of the inner surface of the pan?

This objection seems to have occurred to the author; for he admits that sometimes poisonings produced by utensils with worn tinnings are not caused by saturnine colic. These, he allows, are attacks of gastritis and enteritis induced by the introduction of a considerable quantity of lead into the stomach, or the mixture of some parcels of lead conjoined with other poisonous substances, for instance the oxides of copper.

The author takes a view of the noted subject of the sophistica- on of eager sharp wines by means of lead. But neither in this or in his facts upon the contamination of water by means of ad is there any thing new or peculiar.

The author could not probably be aware of the facts as to the ac- n of wine upon lead brought to light by Dr Alfred Taylor; for we mistake not, these facts were made known subsequent to the pearance of the work of M. Tanquerel. Dr Taylor, it may be proper to mention, found that good wine, whether port or sherry, becomes impregnated with lead only very slowly. After leaving leaden-shot in quantities of port wine and sherry for months and years, Dr Taylor found that the port after several months retained its colour and had dissolved scarcely any portion of the lead. The sherry, however, had become darker in colour, and the presence of lead in it was manifest. After the lapse of six years the port wine had still a dark red colour, and gave to hydrosulphuret of ammonia and sulphuretted hydrogen gas only faint traces of the presence of the metal. The sherry, on the other hand, had acquired a pale straw colour, and was pretty strongly impregnated with lead.

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* Annales d' Hygiene et de Medecine Legale. Tome x.

From these facts Dr Taylor draws the conclusion that good wine, that is, we suppose, wine duly fermented and free from acidity, is but slowly contaminated by lead in the metallic state; that white wine is more than port wine; and that very acid wines, as those made from currant or gooseberry, may be readily and abundantly impregnated with the metal. It is further likely that these conclusions apply to the light acidulous wines of France.

It is known that, under certain circumstances, water conveyed in leaden tubes and retained in leaden cisterns, dissolves part of the lead and thus proves poisonous; and, in other circumstances, it either dissolves no lead, or the lead is precipitated, and the water becomes harmless.

It is chiefly atmospheric air and carbonic acid that are the agents in bringing about these changes. Lead exposed to water is first converted into an oxide or a hydrate, and is then converted into a carbonate by the carbonic acid present in the water. This change takes place most easily and rapidly in the purest waters. Thus in rain water and soft water generally the change is liable to take place; and in distilled water the formation of the hydrate is easily and rapidly effected, providing the atmospheric air has access to the water. After the formation of the hydrate by the joint action of air and very pure water upon the metal, another change is liable to be effected by the action of carbonic acid gas. According to Dr Christison, who has examined this subject with great accuracy, it is not pure carbonate of lead that is formed when metallic lead is traversed by, or in contact with, pure water. It is a permanent compound formed of two equivalents of carbonate of lead and one of hydrated oxide of lead.

In the second place, it is ascertained that water containing saline matters, especially sulphates and muriates, does not so readily act, and is not acted upon by metallic lead. Thus hard waters, which owe their hardness to sulphate of lime, are not at all acted upon by lead; the metal forming a sulphate, which is also insoluble, and is ascertained to exert no appreciable action upon the human body. This protective power, or non-solvent action of water upon lead is possessed in the strongest degree by sulphates and carbonates, in a slighter degree by muriates. Thus carbonate of lime or carbonate of soda may equally render water incapable of being acted on by metallic lead; but muriate of soda and muriate of magnesia are less useful in this way, and allow lead salts to be formed. A mixture of equal parts of river water and distilled water exerted no action on lead; consequently, infers Dr Taylor, a preservative power is exerted where the proportion of saline matter could not have been greater than one-fourteenth thousandth part. The Edinburgh water, which contains, according to Dr Christison, about one-twelve thousandth part of saline matter, has but a feeble action upon metallic lead. The same authority thinks it a just and safe conclusion to establish, that water which contains less than an eight thousandth part of salt in solution, cannot be safely conducted in leaden tubes without adopting certain precautions to prevent chemical action. This proportion will prove insufficient to prevent chemical action, unless a considerable part of the saline matter consist of carbonates and sulphates, especially the former. When the salts in solution are in a great measure muriates, a proportion so large as a four thousandth part, or even more, will be insufficient to prevent all mutual action.

The knowledge of these facts has led to various useful inferences regarding the circumstances in which lead, which is conveyed in leaden tubes, and retained in leaden cisterns, communicates to the water poisonous properties. When leaden tubes are newly laid, and when water is received in a cistern lined with new lead, it often happens that the hydrated oxyde and carbonate of lead are necessarily formed at first; and corresponding to this formation, it has been known that accidents of saturnine poisoning have resulted. These accidents, however, although they take place during the first employment of the tubes and cistern, gradually cease, and do not again recur. The reason, it is said, that they do so, is because, after the first formation of carbonate of lead, the lead ceases to undergo any further change, and no more carbonate is formed to be removed by the water. It is conceived, therefore, to be injurious to clear away from the interior of lead tubes or cisterns the incrustation by which they are lined.

A source of contamination of water in cisterns was first observed by Dr Paris, and has been admitted by Dr Pereira and Dr Taylor. When the cover of a cistern is lined with lead, the water which rises by evaporation, being pure and free from saline matter, exactly like distilled water, acts with certainty and expedition upon the lead lining, and forms the hydrated oxide and carbonate, which, again dropping into the water below, may sufficiently impregnate that to render it poisonous. The rule to be observed as deduced from this fact is, that the covers of cisterns should be of timber only, without any lead lining.

At the same time, we believe, that the most effectual way to guard against all accidents of this nature, from water remaining any time in contact with lead, is to abandon entirely the use of leaden cisterns, and to employ stone or marble in the construction of these receptacles.

When any other metal is joined with lead, as iron, copper, or zink, Dr Paris thinks that galvanic action may take place, and may thus operate most speedily in producing water containing salts of lead, which shall exert a poisonous action upon those by whom they are used.

Certain accidents in the way of lead poisoning appear in the manner now mentioned to take place with very pure water, that is, water free from carbonates or sulphates. Thus at Tonbridge some years ago accidents of lead poisoning appeared among the inhabitants. These chiefly were dependent upon the purity of the water supplied, which it is said contained only a thirty-eight thousandth part of saline matter,—a proportion very much below the protecting rate.

At Ascot it is stated the water is exceedingly pure; and this degree of purity has been usually assigned as the reason why the water became so readily impregnated with lead as to cause colic, palsy, and atrophy among the hounds of the royal kennel at that place.

One of the most remarkable instances of lead poisoning on several accounts, which has taken place of late years, is that which occurred in the family and attendants of the late Count de Neuilly, ex-king of the French, at Claremont in October 1848. In the beginning of October 1848 there were at Claremont thirty-eight inhabitants. Of these, thirteen persons had been attacked with symptoms of lead poisoning, mostly in the form of *Colica Pictonum.* As the water was suspected, it was examined by Mr Hoffmann, and found to contain a considerable quantity of lead. This water further there was strong reason to believe owed its saturnine impregnation to the great purity of the water. The circumstances however deserve to be known.

The spring which furnishes the palace of Claremont with water, issues from a bed of sand about two miles distant. This spring was selected from among a great many other springs in the vicinity, by reason of its uncommon purity; and the water was thirty years ago conducted to the palace through leaden pipes. "In the present day," observes M. Gueneau de Mussy, "some other metal would probably have been selected; for experience has taught us that pure water, and especially destilled water, acts rapidly on lead when it comes in contact with the metal."

"Thus Tronchin," he continues, "proved that the inhabitants of Amsterdam were indebted to the rain water kept in leaden cisterns, for the colic, to which they were in his time so much subject. The purity of the Claremont water becomes a most dangerous property not only to it but to other springs. Whilst I was combating its pernicious effects, I heard that there had been several similar cases in different parts of England. They are not uncommon in the county of Surrey, and especially in the neighbourhood of Claremont. Besides the cases published by Dr Thompson of Stratford-upon-Avon, I know of several others at Weybridge, Windsor, and in different other places."

The late Mr Richard Phillips had analyzed the water of the Claremont spring, and found in one gallon of it only 5·7 grains of solid and saline matter, consisting of

	Grains.
Common salt, ...	2·7
Sulphate of lime, silica, oxide of iron, and vegetable matter, ...	3·
	5·7

As this is a proportion of salts, especially sulphates, too small to prevent the formation of carbonate of lead, or less than one-four thousandth part, M. Gueneau de Mussy thinks that this character of the water is the main cause of its acquiring the leaden impregnation.

He allows, nevertheless, that this does not account for the previous immunity from poisonous effects, and the occurrence of these effects in October 1848. If this had been the cause then the water ought to have shown its poisonous agency long before, and especially at the time at which the tubes were first laid. He accordingly mentions the following circumstance as the only one deserving attention, which he had been able after careful inquiry to discover.

Until about eleven months before the occurrence of the poisoning accidents, leaden tubes were employed to take up the water from a large natural cistern near its source. When the present occupants of the palace came to reside in it, they wished to preserve this natural cistern from the vegetable and animal matters that usually fell into it. For this purpose an iron cylinder of six feet in diameter and twenty feet high was constructed, and was sunk into the ground fifteen feet deep. The water welled up within, and a leaden-pipe was attached to it with a funnel-like mouth projecting by a few inches in the inside of the cylinder, which was closed at top by an iron cover with several holes in it, to allow the air to make the requisite pressure on the surface of the water.

M. Gueneau de Mussy thinks that the part acted by the iron-cylinder in this case was three-fold. First, it induced galvanic action between itself and the lead, both metals being in contact with the water. He allows, however, that he has some hesitation in admittig this explanation; because, were it correct, the water contained in the cylinder, he thinks, should be charged with more lead than that which ran through the pipes, and remained for some time in the leaden cistern of the palace. Now, the truth was, that the water of the iron cylinder contained no lead; the water of the pipes contained some, and that of the cistern contained much more lead.

The second mode in which the cylinder acted was by the chemical action of the iron on the water, and on the salts which it contained.

Thirdly, M. Gueneau de Mussy believed that the purification of the water caused by its inclosure in the iron cylinder, and the filtration which it underwent previously to entering it, tended to cause it to act on or be acted on by the lead more readily than would otherwise have been the case.

The quantity of metallic lead amounted to one grain per gallon; certainly a large proportion when it is remembered that the poisoned water was employed in all culinary and table purposes, and previously to the discovery of its deleterious properties, in the preparation of ptisans and enemata.

We return to M. Tanquerel.

Absorption by the respiratory mucous membrane is a channel not uncommon for the introduction of lead. It is by this channel that persons sleeping in newly-painted apartments receive the poison, and are attacked by its consequences. By some we believe this mode of introduction has been doubted and denied. But M. Tanquerel adduces conclusive evidence to show that it occasionally takes place; and if it take place only once, the fact is proved as well as by one hundred instances.

It may be also remarked, that if oxygen and carbonic acid with pure water act so readily upon lead, it is not difficult to see how easily this action may take place in the bronchial tubes and vesicles.

From the forms in which the metal may be introduced, the author proceeds to the trades and professions who suffer most by this poison. It is chiefly the members of those trades and professions who furnish the greater number of instances of lead poisonings and lead colic. These results are not very frequent either in consequence of the use of lead medicines, or the use of poisoned drinks or poisoned water. To communicate some idea of the nature of these professions and the numerical proportions in which they are affected by lead diseases, M. Tanquerel presents the following list.

STATEMENT

Of the trades and professions of twelve hundred and thirteen individuals labouring under symptoms of lead colic.

Trades.	Number of Cases.
Workmen at White Lead Manufactories,	406
Workmen at Red Lead Manufactories,	63
Workmen at Manufactories of Orange Lead,	12
House Painters,	305
Coach Painters,	47
Decoration Painters; Letterers and Sign Painters,	33
Porcelain Painters	3
Gilders,	1
Metal Painters or Japanners,	2
Stained-Paper Makers,	2
Colour Grinders,	68
German Card Makers,	13
Glazed Card Makers,	6
Waist-Belt Makers,	2
Perfumers,	2
Earthen Ware Makers,	54
Dutch Ware (Delf) Makers,	7
Refiners,	25
Lead Founders,	14
Tinworkers, (Pewterers),	8
Solder-Makers,	4
Tinsmiths,	4
Jewellers, Goldsmiths,	4
Copper Founders,	2
Bronze Founders,	1
Workmen at Type Founderies,	2
Printers,	52
Workers at Leaden Shot,	11
Lapidaries,	15
Cutters and Polishers of Crystals,	3
Workmen in Glass Manufactories,	2
Workmen in Sugar of Lead Manufactories,	4
Workmen in Nitrate of Lead Manufactories,	3
Workmen in Chromate of Lead Manufactories,	3
Total Amount,	1213

SPECIFICATION

Of trades different from those mentioned in the preceding list, in which attacks of lead colic have occurred; numbers not given.

Workmen at Lead Mines.
Workmen at Litharge Manufactories.
Stokers in Steam-Vessels.
Glaziers.
Porcelain-makers.
Glass-makers.
Enamel Manufacturers.
Druggists.

These lists indicating the occupations in which workmen have been seen labouring under saturnine colic, present a general summary of the principal positions in which men may ordinarily be placed, when they are attacked by this disorder. Other trades and occupations probably there are in which lead colic more rarely takes place. These, however, notwithstanding all his means of investigation, M. Tanquerel has not been able to discover, either at Paris or in the provinces.

The Hopital de la Charité, which has long been a place of resort to patients in this disorder, receives indiscriminately the members of every species of trade, which are affected by lead colic, and who, attracted by the celebrity of its long-established treatment, come by preference to be treated in this establishment. This circumstance induces the author to think that the table here given must indicate with considerable precision, the comparative frequency of this disease, considered in relation to the occupations carried on in the city of Paris.

He thinks it not doubtful that more than twelve hundred and four patients suffering under this disease have come for advice, or have been received into the wards of La Charité during the eight years which have elapsed between 1831 and 1839. Some may have escaped examination; but this difficulty applies indiscriminately to patients of different conditions.

The Hospital De La Charité receives a greater number of patients labouring under lead colic than all the other hospitals of Paris taken together.

It would be impossible, with nothing but the facts furnished by the registers of La Charité, to establish a return like that here given without falling into serious errors. This source of them is indeed little to be depended upon. This arises from the following circumstance. Of the workmen engaged in the white lead and red lead manufactories, many have two occupations, one habitual and original, the other occasional and adopted. At the white lead and red lead manufactories they work only occasionally when they cannot find employment at their habitual and original craft. From this it follows, that when these persons, the most numerous class of the patients who apply at La Charité, come to the hospital labouring under lead colic, in almost all cases they give their names for inscription in the Hospital Register, with the designation of their original profession; for instance, mason, plasterer, earth-worker, distiller, mariner, soldier, and so forth.

In the returns to be made from facts thus collected, it would be wrong to place to the charge of one of these original occupations a disease which it could not have caused. This, however, M. Tanquerel argues is what has been actually done by trusting implicitly to these registers, in so far as M. Merat and various others have thought, that lead colic might proceed from the effect of many other substances besides lead.

It is enough to say M. Tanquerel des Planches has been aware of this source of error, and has avoided it.

A difficulty of a different kind though not unallied to this has presented itself in these etiological investigations.

A female was received into the Hospital de La Charité under M. Rayer, labouring under symptoms which bore a perfect and exact resemblance to those of lead colic. This female, however, was a portress, and asserted that she had taken no medicine for from five to six years, that she had never dwelled in newly-painted rooms, and had in no manner worked in lead. In short, all the questions proposed were unable to prove that the woman had been in contact with lead. The fact appeared to M. Tanquerel so extraordinary, that he resolved to employ all means in order to discover the cause of the disease. He repaired to the abode of the patient in the *Rue des Petits-Augustins*. She occupied an upper room, the floor of which was in very bad condition, and was perforated by many holes. Immediately below this floor in the ground-flat was the laboratory or workshop of a perfumer. In this place were pounded and sifted, at the beginning of every month, several pounds of white lead, in order to be incorporated with several cosmetics, pomatums, &c. During this operation part of the air of the laboratory, loaded with particles of white lead, readily passed through the fissures and holes of the floor of the room inhabited by this female.

It was therefore not surprising that this woman should be attacked by lead colic. The gums and teeth of this patient presented in a small extent the blue line, denoting sulphuret of lead. Had this case not been made the subject of careful inquiry, it might have been supposed that the colic proceeded from some other cause.

1. *Workmen in White Lead Manufactories.*

The earliest manufactories of white lead, or subcarbonate of lead, according to the author, were established in France sixty years ago about the year 1791, by M. Chaillot of Paris. From that time lead colic became a disease greatly more frequent than formerly.

The most noted manufactories of white lead established in France are those of Clichy and Courbevoie near Paris, of Lille, Strasbourg, Orleans, Lyons, Dunkirk, Marseilles, Pezenas, and Nantes. The number of persons engaged in these works arises at least to five hundred,—men, women, and children. In Germany, Holland, and England are also a considerable number of manufactories of white lead.

In all the white lead manufactories of Europe there is prepared an enormous quantity of subcarbonate of lead; because the substance is extensively employed in the arts, because it forms the basis of almost all the mixtures used in oil-painting.

In 1831, that is, at the time at which M. Tanquerel des Planches commenced his researches upon saturnine diseases, there were in the Department of the Seine, and the Seine-and-Oise, five manufactories of White Lead. The workmen who were taken ill were sent to the hospitals of Paris, and especially to that of La Charité.

	Workmen.
Clichy employed - - -	50
The two manufactories of Le Pecq, -	30 to 35
Courbevoie, - - -	20
Port-a-l'Anglais or Vaugirard, -	6
In all, -	106

In 1839 the two manufactories of Le Pecq, as well as those of Port-a-l'Anglais and Vaugirard, had ceased from active operations. Hence at and some time previous to this period, 1839, patients came only from Courbevoie and Clichy.

The works, nevertheless, are not in activity the whole year round. For whole months only a few workmen are employed in the mere commercial department only.

Notwithstanding this small amount of active occupation, the class of white-lead makers at Paris, though not numerous, furnish an amount of cases of lead colic equal to one-third of that of the total population of all the states who employ lead, however extensive it be elsewhere. The cause of this great prevalence of the disease M. Tanquerel finds in the nature of white lead and the processes of manufacture.

Subcarbonate of lead cannot be converted into vapour. But it is easily reduced into a very fine powder or dust by mere handling. Friction, percussion, scraping, braying, in one word, all those operations, the object of which is to break and dissolve the aggregation of its particles, have the effect of dividing it into particles so light, that they are disseminated with the greatest facility through the atmosphere.

The most usual processes employed for the manufacture of white lead are the following; the Dutch process; the French process; the process of Reboul or the Austrian method; and the process of Torassa and Walkerwood.

The Dutch method, which is the oldest known, consists of the following series of operations.

First, the lead is melted and formed into thin plates.

Then after spreading a layer of manure, in some places tanners' bark is used, over this conical earthen vessels containing vinegar are placed in rows, and on each vessel a thin plate of lead. Over this is placed a sort of floor or covering of deal. Above this is spread another layer of manure, with rows of earthen vessels containing vinegar, and each covered by a thin plate of lead. Successive layers of this construction are formed, until the chamber in which this operation is arranged, is sufficiently filled.

It is understood that the lead is first converted into acetate. But while this is proceeding, the fermentation of the manure gives rise to the formation of carbonic acid, which unites with the lead and forms a subcarbonate.

At the end of about one month, as soon as the surface of the lead has been well converted into subcarbonate, this subcarbonate is removed from the metal by striking the plates with a wooden club.

This beating process is sometimes insufficient to detach all the subcarbonate formed. The men are obliged to pick off these adherent portions with their hands. This operation, in short, is known among the men by the name of picking.

The residual lead plates not carbonated are again melted to be used in the same manner. In some instances, however, especially where tanners' refuse is employed, and the lead is exposed a sufficient time, the whole is thoroughly converted into subcarbonate.

The carbonate thus detached is put through the mill, then boulted, and finally sifted.

The white lead is then put up in small casks and is ready for the market.

When it is wished to prepare white lead, which is esteemed very fine, the finest part is selected and diluted with water. The paste thus formed is subjected to another grinding between horizontal millstones.

The white lead, still moist, pasty, but sufficiently triturated, is put in pots which are placed in a stove to be dried. This lead is then wrapped in paper, and is ready for the market.

This is the method pursued at Courbevoie.

In all these operations a considerable disengagement of saturnine dust or fine powder takes place. Some of the operations are more dangerous than the others. Thus the beating and picking are both dangerous, because dust is produced by the operation. But these are as nothing compared to the grinding, the boulting, the sifting; and, in the making of very fine lead, the second milling and the potting. The second milling process, which is done with the aid of water, M. Tanquerel regards as less dangerous than the others. The fact that white lead in powder is detached and diffused through the atmosphere has been also proved experimentally. This was scarcely requisite: for simple observation is sufficient to show that the dust is readily carried in this manner through the atmosphere of the places in which these operations are conducted.

Almost the whole surface of the bodies of the workmen is covered with particles of white lead. It may be divided into a white dust so fine, that it is not perceptible by the naked eye in the cutaneous furrows to which it adheres. But if these points be touched by sulphurous water, the presence of lead is instantly indicated by the black stain. Some of these men even who have as a matter of cleanliness washed the surface of their persons carefully with soap and water have apparently cleared away all traces of lead; but, upon bathing in a sulphurous bath, the true state of the skin was shown by the dark colour which it speedily assumed. This dark tint was most conspicuous and distinct at the nails, at the forepart of the thighs, at the level of upper end of the breastbone, on the chest, over the navel, and at the palmar surface of the hands.

In the French method, which is practised at Clichy, litharge, or the protoxide of lead, is exposed to impure vinegar, and through the watery solution of acetate of lead thus formed a current of carbonic acid gas will be transmitted.

In this way the carbonate of lead is obtained in a moist state, and retaining still a little undecomposed acetate of lead. In this condition it is carried by means of a sucking pump into large troughs, where it is thoroughly stirred and washed, in consequence of which the acetate is washed away through channels made in the walls of the troughs, while the ceruse is left behind in the solid state.

The ceruse is then filtered.

After this it is placed, still in the form of a moist soft paste, in pots where it is exposed to a gentle heat in a stove. In this state it is in the form of loaves ready for the market.

If it is wished to have it in the form of powder it is passed through the mill, then through the sieve, and then put up in casks.

In this method various steps are attended with danger.

First, the carbonic acid gas, which is always in excess, and heavier than the air, comes in contact with the beds of litharge, and carries away very minute parcels of acetate and carbonate of lead. The extrication of carbonic acid gas and the lead emanations are above all abundant when the large tun is opened in order to allow its contents to be conveyed to the troughs for stirring and washing

Secondly, M. Tanquerel regards as a dangerous operation the cleaning of the pots, which previously contained the ceruse. During this cleansing, which is effected by scraping from the walls of the vessels the dry adherent ceruse, the workman is exposed to a large quantity of saturnine dust. With this his clothes, his person, and members are covered. When the pots are once cleaned, the workman packs up the moist ceruse. The danger here is greatly less.

Thirdly, The drying process in the stove may be considered as extremely pernicious. The large quantity of water which the *Clichy* white lead always contains, is easily evaporated at a temperature as high as that of the stove. This water in undergoing the conversion into vapour, the author thinks carries with it into the atmosphere particles of ceruse and undecomposed sugar of lead. Any one who looks into the drying chamber for some minutes recognizes a strong odour of acetate and carbonate of lead.

Fourthly, The braying, the sifting, the barrelling, of the Clichy white lead, which are performed in the same manner as in the manufactories conducted on the Dutch method, are in like manner very dangerous operations. In short, multiplied evidence shows, that wherever the workmen are exposed to the dust of white lead, or to white lead in dust and powder, there the substance is absorbed by the respiratory and digestive surfaces, probably by the skin, and gives rise, with promptitude and a considerable degree of certainty, to all the usual effects of lead poisoning.

It is unnecessary to notice in detail the other two methods, that of M. Reboul at Pezenas, or the Austrian, and that of MM. Torassa and Walkerwood. Both consist in converting metallic lead into carbonate, though in the latter way neither is vinegar employed, nor heat, nor expensive apparatus required. The lead is acted upon by water and the atmospheric air, the carbonic acid of which is stated to be sufficient to effect the conversion into carbonate.

The other manipulations of picking, braying, drying, sifting, and packing, are all much the same, and all in the same degree and mode dangerous.

Opinions are much divided upon the greater or less degree of facility with which each process induces lead colic.

Each manufacturer affirms, without hesitation or comparative view, that his own method is the most safe; and physicians not following the workmen in the subordinate details of their labours can do little more than receive and assent to the representation of the overseer or director. The author repeatedly visited the works of Clichy and Courbevoie, and interrogated the workmen who had left these works as patients, with the purpose of ascertaining the relative numbers of men where white lead was made according to the French and Dutch methods respectively. These inquiries have furnished the following results.

Among the four hundred and six white-lead makers specified in the table, two hundred and ninety-four had worked at Clichy, eighty-eight at Courbevoie,* fifty at Pecq, and twenty at Port-a-l'Anglais or at Vaugirard. It must be remembered that Clichy employs nearly three times the number of workmen more than Courbevoie, so that it is not wonderful that this last manufactory has sent less than one-third of the patients. From the other works in which the operations were continued not more than two or three years, no accurate calculation can be formed.

The white lead makers, who work according to the French or the Dutch method, contract lead colic after continuing at these establishments for a space varying from three days to six years.

Of the workmen observed by the author, those coming from Clichy have worked on an average term fifty-one days before being attacked by colic; those from Courbevoie have ceased on an average after fifty-seven days' work. This difference in the average number of days, which a workman may be exposed in each of these establishments, the author thinks is sufficient to justify the inference, that in one of these manufactories the men are more speedily affected than in the other. He adds, however, that no positive conclusion can be drawn as to the greater hurtfulness of the French method than the Dutch; for he observes there are, besides the modes of manufacture, other circumstances which render the danger relative.

For example, he continues, at Clichy, twenty times more ceruse is made than at Courbevoie, and consequently he thinks the workmen should more rapidly be taken ill.

Further, at Clichy all the processes are conducted in the same

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* There is some error in these numbers, but we cannot venture to correct it. Taken altogether, they make not 406 but 452. Thus—

Clichy,	...	..	294
Courbevoie,	...	...	88
Pecq,	...	...	50
Port-a-l'Anglais, &c.,		...	20
			452

The number 294, which most probably is not right, would require to be 248 in order to give the sum 406.

house; all the workmen are in contact with the emanations issuing from the whole body of white lead which is manufacturing in one single workshop. On the other hand, at Courbevoie, the processes are conducted in separate apartments, and consequently they are not exposed to the contact of the whole dust of the establishment. This difference in arrangement, M. Tanquerel thinks, ought to exert a considerable influence upon the health of the workmen.

The general correctness of these inferences, M. Tanquerel thinks, is proved by the fact, that for one year previous to 1839, at Clichy, a small quantity of ceruse was made according to the Dutch method, in the same workshop where the ceruse was manufactured by the French method. M. Tanquerel remarked that the workmen were always attacked at a more early period than those of Courbevoie. He thinks it further probable that the ceruse of Clichy, obtained as it is by the French method, being lighter than that of Courbevoie from its containing sugar of lead in large amount, may be more easily disseminated through the air, and hence may more readily affect the workmen. It has been also observed that in the different trades, as house painters and others, who make use of white lead in large amount, the white lead of Clichy occasions more frequently and more easily lead colic, than that which has been prepared according to the Dutch method.

If the statements of M. Reboul, the proprietor of the manufactory at Pezenas, on the other hand, can be believed, lead colic is a greatly less frequent disorder there than either at Clichy or at Courbevoie. Though M. Tanquerel thinks that this gentleman's assertions as to the unfrequency of colic at Pezenas are not free from exaggeration, he allows that the workmen are there less exposed to the injurious operation of the white lead than elsewhere. He observes, that as the white lead, when withdrawn from the stoves, is in the moist pasty condition, and does not require to be brayed, but merely crushed, dried, and put in packets, there is much less room for the inhalation, deglutition, and other modes of introduction of the poison, than where the white lead is ground or brayed, boulted, sifted, and passes through all the states which a fine dry powder must pass through.

In conclusion, it is an irresistible inference, that among all the workmen who have occasion to handle saturnine preparations, the white lead makers are those who are most frequently affected by lead colic. The managers, superintendents, and overseers of these establishments are themselves attacked by the disease. In no other profession are men so incessantly and thoroughly surrounded on all sides by so large and fully impregnated an atmosphere of saturnine emanations. These persons breathe, so to speak, lead, and they may swallow it every moment.

2. *Manufactories of Red Lead.*

Several manufactories of red lead are established in France. That of M. Pecard Tachereau at Tours is the most considerable.

In the department of the Seine are three red lead manufactories; those of Clichy, of La Villette, and of La Rue d'Aval. In these works twenty workmen are employed in the manufacture of this oxide of lead. The number of persons attacked by colic is not so great as among the white lead manufacturers. But it will appear that the red lead makers are more frequently attacked by saturnine arthralgia than by colic; from which it results that they are more frequently attacked by a lead disease, abstracting from the particular species than the white lead workmen.

The workmen employed in making red lead scarcely work upon an average beyond forty-five days, before they are attacked by colic or saturnine arthralgia.

The processes for manufacturing red lead consist of a numerous series of manipulations. The one pursued at Clichy is the following.

Into a reverberating oven is introduced metallic lead in salmons, as they are technically called. A fierce flame, raised from wood-charcoal, or pit-coal, to cherry-red, causes the metal to melt and be calcined. When the metal is melted, the calciner, by means of an iron rake, agitates the melted lead thoroughly, in order to convert the whole into a powdery mass of oxide of lead. During this operation, which lasts seven or eight hours, the workman is incessantly at the throat of the oven, from which issues a strong dense smoke of oxide and of metal reduced into powder and vapour. Meanwhile part of the air and of the gas which escape from the furnace is received into a long chimney, situate under the dome, and which conveys to a distance part of the emanations.

When the lead appears to be entirely converted into oxide, the fire is allowed to go out, and the oven is shut.

The product of calcination remains in the oven till next morning. It is then removed quite cooled, by means of a long iron shovel, and it is conveyed to the washing mill. This conveyance of the powdery oxide is not done without diffusing through the air a certain quantity of dust.

The object of washing is to bray the protoxide or massicot, and to separate it from the metallic lead with which it is still mingled. In order to obtain this twofold result, the braying mill is mounted within a tun into which water is forced by a sucking pump. In proportion as the massicot is brayed, as it is lighter than lead, it is separated from it, and suspended in the water which carries it off.

As soon as the water of the mill loses its yellow colour and begins to grow black, the pump is stopped. The metallic lead left behind is taken out and again placed in the oven for another calcination. An hour and quarter, or one hour and a half, are usually sufficient for one washing operation.

When the workmen take care not to agitate the massicot in placing it in the tun, they do not disseminate the saturnine dust during this operation. But their hands are often in the washing tun, where they come in constant contact with the massicot.

After the massicot has been duly brayed and washed, it is conveyed to the drier or kiln, situate above the calcining oven. This drying kiln is surrounded with a wooden parapet seven or eight inches high. The liquid massicot poured into this receptacle is at the end of two or three days sufficiently dry. During the desiccation a thick vapour or smoke, consisting of numerous particles of moist massicot, arises from the kiln. The workman, when he removes the massicot from this place before it is completely dry, is in the midst of an atmosphere saturated with leaden particles.

The massicot removed by means of shovels is placed in cisterns of beat iron for a second burning or calcination, which receives the particular designation of *reverberation.* With this intention the workman arranges the cisterns in such a manner that they are piled on each other to the ceiling. All access to the external air is excluded as much as possible. For this purpose the workman closes the register of the chimney, the mouth of the oven, the doors of the ashpit, and the fire-place. Hitherto the calciner is very little exposed to the contact of particles of oxide of lead. But next day, on opening the oven, a great quantity of saturnine dust is disengaged in consequence of the currents of air which rush on all sides into the oven.

The cisterns, when removed, contain a product of a colour already intensely red. This, which is called *one fire red lead*, is a mixture of protoxide and deutoxide. It is then reduced to powder in a boulting machine, consisting of two cylinders, one placed above the other. Though this braying machine is surrounded by a wooden case, a great quantity of dust escapes and surrounds the workman.

Thus pulverised dry, the *one fire red lead* is subjected to another reverberation in the iron cisterns in the oven as at first. This roasting converts it into *two fire red lead*, or the red lead of commerce.

The final operation is the barrelling, perhaps the most dangerous of all. It is easily understood.

There is another species of red lead, or oxide of lead, known in commerce under the name of *orange mine*, or *English mine.* This is ceruse or white lead reverberated two or three times after the manner of massicot.

The description now given will enable readers to understand the dangers of this occupation, and the mode in which the workmen are poisoned. The most dangerous operations are placed in the following order:—the barrelling, the reducing to powder, the first calcination, the kiln-drying, and the reverberations. In all these several operations the poison is placed in contact with the mucous surfaces of the respiratory and digestive passages, and with a large extent of the skin. The workmen complain of the saccharine taste, and of the specific odour of red lead; their teeth and gums are covered with sulphuret of lead. Their skin is dyed red by the oxide.

3. It is unnecessary to enter into the details of the Litharge Manufactory. It is sufficient to say that this article is obtained by heating lead to a white heat. The calcination or cupellation, as it is often named, is attended with a little more danger than the same operation, as it is conducted from red lead. In other respects, the braying, the sifting. and the barrelling, are all equally productive of mischief by diffusing minute lead-dust through the atmosphere of the apartments, in which the men are working.

4. *House painters*, in all their departments, form the class of painters most numerously attacked by lead colic. From various authentic documents, the author estimated the numbers of this occupation in Paris, at five or six thousand persons, who work annually in the city, and who, in favourable seasons, as spring, summer, and autumn, are in the daily exercise of their calling.

Among house painters, who exercise their craft for the space of thirty or forty years, one-fourth nearly are attacked during this time with lead colic.

Some workmen work scarcely a few days when colic shows itself. Others, on the contrary, have no attacks for twenty or thirty years of labour.

Subcarbonate of lead or white lead is the substance which hitherto has formed the basis to almost all sorts of oil paint. Sometimes in addition to this, chromate of lead is added, sometimes red lead or litharge, and other colouring matters; with fixed oil and essence of turpentine. The latter are used as diluents; litharge as a desiccative.

The operation of preparing the colours and mixing the substances is considered to be dangerous. Part of the oil of turpentine evaporating carries with it portions of the lead. It is clear that the turpentine cannot, as turpentine, do harm; for it is often administered internally in large doses with advantage. Here, however, by its volatile character and its property of carrying away minute particles of lead, the mixture becomes poisonous.

The number in the table (305) gives about one in eighteen or nineteen persons.

House painters are in various modes, besides the act of mere painting, exposed to lead emanations. The practice of burning off old paint from walls and doors, which M. Tanquerel says is not unfrequent, and which is done by covering the surface with oil of turpentine and then setting fire to it, is mentioned as a service in which lead emanations are abundantly detached.

It is said, on the other hand, that the use of spirit varnish, and what is called fat varnish, with or over oil painting, protects the artists in a great degree from the hurtful effects of lead. Painting in size seems to cause little danger, though white lead forms a considerable part of the composition.

The colour-grinders, whether as a separate profession, or as dependent on house painters, are frequent sufferers.

5. *Coach painters* are not very frequently attacked by colic. Of these men about one thousand were in Paris in 1839. The number in the table, 47, is about one in twenty-one.

6. *Sign painters, letter painters, and fancy painters* are said all to be much less frequently affected by colic than house painters. These workmen paint on grounds previously prepared by the house painters; but they use infinitely less lead in their operations than house painters. In other respects they perform on the small scale what is done by house painters on the large.

7. *Porcelain painters* are a species of decoration or fancy painters. They employ oil of turpentine, fixed oil, red lead, white lead, and chromate of lead, for painting on vases and other porcelain articles. But the saturnine preparations which they use are in small quantity, and they handle them little. They are not very subject to the disease.

8. *Portrait painters* are not at the present time, and have not for a long time past been very much affected by lead colic. The substances are prepared before hand by others. These artists do not use oil of turpentine. Formerly, it is supposed that they were liable to the disease. A report has been occasionally prevalent that the early death of Corregio and of Raphaele, as well as other celebrated painters, was caused by lead poisoning, and Tronchin has favoured this report. M. Tanquerel differs and entertains doubts. Of the mode of the death of Corregio we have at present no exact information. But Mr Dennistoun,* in his recent interesting work on the Dukes of Urbino, gives on the death of Raphaele sufficiently detailed and exact information, to show that poisoning by lead was out of the question. The professional labours and incessant duties of Raphaele had overtaxed the powers both of mind and body. "A frame naturally delicate, exhausted by mental fatigue, was incapable," he tells us, "of resisting the first attack of

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* Memoirs of the Dukes of Urbino from 1440 to 1680. By James Dennistoun of Dennistoun. Vol. ii. book iv. chap. xxix., p. 230. 3 vols. 8vo. London, 1851.

disease." While superintending, in March 1520, the frescoes of the Farnesina, an urban villa of Agostino Chigi, a wealthy banker of Rome and Sienna, "a summons from the Pope brought him with hurried steps to the Vatican, where, arriving overheated, he was detained in a large chilly saloon, until perspiration was checked. An attack of fever naturally followed, which, advancing to the stage called pernicious, proved too much for his delicate and over-excited frame, especially when still further exhausted by injudicious bleeding, in a belief that the attack was pleurisy. Aware of his danger, he sought support in his hour of need from the ministrations of religion and the rites of his Church."* Raffaele of Urbino breathed his last on the night of Good Friday, 6th April 1520, to the universal regret of all. He was in the thirty-seventh year of his age.†

On the correctness of the criticism expressed either as to the nature of the disease or the alleged hurtfulness of the remedy, we give no opinion. But it is clear that an illness of about eight or nine days could not be caused by lead poisoning. It seems to have been an attack of miasmatic or malaria fever.

9. *Gilders* scarcely can be said to be attacked except when they act as house-painters. They employ white lead and chromate of lead in very small quantity.

10. *Metal varnishers or Japanners* are a species of fancy painters. They employ white lead diluted with fixed oil, essence of turpentine, and spirit varnish. Sometimes they add to their tints chrome yellow. These men are little liable to contract lead colic. There were about 100 in Paris in 1839; the number on the list is two; this is one in fifty. M. Tanquerel states that he had met with only one patient, and four persons who had been formerly ill.

11. *Stained paper-makers.*—These persons employ white lead, chromate of lead, and other colouring matters united with size. They use no essence of turpentine. They are not often in circumstances to contract the disease.

12. *Glaziers.*—Formerly glaziers employed white lead to compose the mastic which they used to fix the squares and panes of windows. They brayed the ceruse at first in the dry state, which caused an evolvement of saturnine dust. At present these workmen employ Spanish white instead of ceruse for the composition of their mastic. Most glaziers, however, are at the same time house-painters, and the author thinks that when their names occur in the registers as glaziers, it is not in this occupation but in that of painters that they contract the disease.

13. *Colour grinders.*—The substances brayed or ground are ceruse and chromate of lead. The operation is fatigueing and pernicious. The operation is performed upon a square table of very hard

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* Ibidem, p. 238.

† Born 6th April 1483.

stone, and over which the workman carries in all directions, in circles and ellipses, a sort of heavy pestle or milling stone, shaped like a truncated cone, the larger base being the one moved over the table, and its surface being slightly convex. The workman is much exposed in this operation to saturnine dust and vapours; yet this is not the worst. Previously to the grinding process he has often to break the loaves of white lead and chrome yellow by a mallet or hammer. This is the most dangerous part of the trade.

Grinders who grind ceruse and chrome-yellow with fixed oil or cold water, after these substances have been coarsely crushed n the dry state, do not, it is said, contract colic. In this condition are grinders of colours for porcelain painters.

Various ingenious mechanical contrivances have been devised to diminish and obviate the dangers to which these men are exposed. Thus mills and grinding machines, enclosed within cases of wood or tinned iron, so as to prevent the escape and diffusion of dust, have been proposed and employed. The result of all these means, and perhaps of others analogous, is that the colour grinders are now much less frequently attacked by lead diseases than formerly.

14. *German and Glazed Card Makers.* The German card makers employ, for putting on their colours, white lead, red lead and litharge, with other colouring substances, suspended in a solution of glue and water. In order to apply the mixture, it must be boiling; and in this temperature it is said much lead is diffused through their work-shops.

Among twenty-four German card makers, only fifteen have not been attacked by the disease. This is above thirty-seven per cent.

15. The *glazed card makers* are also very liable to be attacked by colic. The material is white lead, suspended in size. It appears, however, that in the large glazed card manufactory of M. Hangrand, in the Rue Meslay, it is chiefly the men employed in braying the white lead who suffer by colic.

16. *Perfumers.*—These persons, from using white lead in various cosmetics and requiring previously to reduce it to very fine powder, are not unfrequently attacked. It appears to be chiefly when they are melting the lard and mixing this and the oils with the lead, at a high temperature, that attacks are produced. Handling these pomatums cold produces no inconvenience or danger.

17. *Belt-makers.*—The *belt makers* whiten with ceruse the belts of sabres and numerous other leather accoutrements. The white lead formed with water into a paste is spread by a pencil or brush over the leather. When this covering has had time to dry the workmen rub and brush it forcibly, an operation which causes the dispersion of a certain amount of saturnine dust. They also previously bray the white lead to a sufficient degree of minuteness. Both are dangerous operations.

Formerly cordwainers (shoemakers) were obliged to whiten the heels and even the shoe-leather of certain elderly females. The heel-whiteners contracted colic in the same circumstances as the belt-makers.

18. *Earthenware Potters.*—In all earthenware manufactories,—from the most coarse material to delf and porcelain,—oxides of lead are employed to form glazing to these vessels.

When earthenware has undergone the first baking, it is then glazed in the following manner: a quantity of litharge, red lead or sulphuret of lead, reduced to a fine powder, is mingled with water and ground in a mill with glaze, sand, and colouring matters. With this varnish the workman covers his biscuit or once-baked ware. The ware is then placed in an oven, where this covering is converted by the heat into an enamel.

The ovens are so constructed that all the smoke and emanations escape into a safe or part which receives technically the name of hell.

Potters, in applying this glazing, have their hands incessantly in contact with the saturnine materials. These men, however, do not often contract the disease. The man who conducts the stove or oven, on the other hand, and is often at its throat, exposed to the vapour, is not unfrequently a sufferer.

It appears, however, that earthenware, properly so called, is less made and less used in Paris than in former years. Luxury, it is said, and wealth, have rejected this cheap species of utensil; and Delf ware and porcelain have been required in their place, even for the most common purposes. It hence results that mere earthenware potters are not often the subjects of lead colic and lead diseases.

It is further said that earthenware glazing, when the ware has been sufficiently baked, does not often cause colic. The lead particles, indeed, which enter into the constitution of the varnish, are so firmly fixed and united with the other parts of the vessel, that M. Tanquerel thinks it difficult for them to escape in such manner as to produce injurious effects upon the workmen.

The glazing of Delf-ware is in all respects so similar to that of earthenware that it forms no practical distinction. The delf-ware glazier is not so frequently, as the earthenware glazier, attacked by lead colic.

19. *Porcelain Glaziers.*—As at present, the glazing of this article is effected by means of feldspar both in France and in England, it is unnecessary to dwell on the subject.

What is called *tender porcelain*, however, in England, is always enamelled with oxide of lead, and this may give rise to the disease.

20. *Glass-workers.*—The oxides of lead facilitate vitrification,

and give to glass thus formed a weight, softness, and a degree of cleanness which no other substance can communicate. Coloured glass, crystals, and glasses for optical purposes, are those which contain the largest quantity of lead. The optical glasses of Faraday contain borate of lead. Silicate of lead performs an important part in the manufacture of crystal, of strass, and other dense glasses for optical purposes.

Workmen engaged in this manufacture are nevertheless soon after, and in great numbers, sufferers by the disease.

Enamel-workers.—These are very little affected.

21. *Stokers and Engineers in Steam-vessels.*—Many of the firemen and enginemen in the steam-vessels sailing from Havre have suffered from the lead colic. According to Duchemin, the cause is that these men employ a mastic or putty consisting of carbonate and deutoxide of lead.

22. *Lead-mine workers.*—France has several lead mines. The most celebrated is that of the Poullaouen in Brittany.

Lead occurs either as a sulphuret (galena, lead-glance), which is the most common, or as a neutral carbonate, as at Poullaouen, or as a phosphate. It is the burning and smelting of these ores that prove dangerous occupations to the workmen. This and the previous crushing and grinding of the ores often give rise to lead colic.

23. *Plumbers.*—These workmen are often attacked by the disease in Paris. In this country they seem to be a healthy class, and are not often the subjects of lead colic.

On *Tinsmiths*, *Pewterers*, and *Tinsolderers* nothing peculiar is communicated. Though all are exposed, they do not often contract the disease.

24. *Type Founders.*—These workmen are distinguished in Paris into two orders. One consists of the *founders,* properly so called, by which is meant the melters and preparers of the type-metal. The other is the order of type casters. Both are exposed to lead emanations; but the former in a greater degree than the latter. The latter, it is said, have the habit of preparing their food and taking it in the same apartment in which the type metal is melted.

Among one hundred type casters only three-fourths have not been attacked by painter's colic. This is to say, that one-fourth or twenty-five per cent. have suffered from the disease.

The types after being cast require to be smoothed and polished. Of all the persons engaged in type-founding, the polishers or rubbers are by far the most frequently attacked. Among one hundred and ten polishers at the type-foundry of M. Tarbé, M. Tanquerel found thirty-five who had been already attacked by lead colic. This is more than one third. The operation is generally performed by females.

The *type-composers* or arrangers differ from what are called compositors in a printing house. To this class of workmen belongs the duty of arranging according to rule the types, which have been cast, rubbed, and polished. Though these men handle the types newly formed, they never contract the disease. M. Tanquerel could not discover one instance in the extensive establishments of M. Tarbé and M. Didot.

Printers are not much exposed to the disease, at least they do not often contract it.

25. *Lapidaries* are indebted for the attacks of lead colic which they occasionally suffer to the wheel of metallic lead which they employ in cutting various precious stones. The rim of the wheel is first covered with emery powder made into a paste with water. This at first prevents much dissemination of lead particles. But after some minutes the workman forcibly brushes his cut stones; the emery being dry particles of lead and emery are readily diffused in the atmosphere in the form of thick dry dust. Sweeping the workshop is another source of diffusing leaden dust. The cutting is not a dangerous operation, but the subsequent brushings and sweepings are very much so.

There were not more in 1839 than fifty workmen. Among these fifty the author met with not more than six persons who had never been affected by colic or other saturnine disease. All the other lapidaries who had been working for one or more years whom he interrogated had been suffering under symptoms of lead poisoning.

26. *Cutters and polishers of crystals and glass workers* are not much affected by lead colic. Among fifty of the latter the author found scarcely one who had suffered from the disease.

Upon manufacturers of nitrate of lead, the salt used in making heavy optical glasses, according to the method of Faraday, and manufacturers of chromate of lead, no precise information is communicated.

The length to which this article has already extended warns us, however, that we must not at present dwell longer on the subject. Much yet remains to be done in order to communicate any thing like a just idea of the labours and services of the author, or of the importance and difficulty of the subject. All that has been now done is merely to give the preliminary views as to the causes of lead-poisoning, the channels and modes by which it may be introduced, so as to give rise to its specific effects. This introductory matter, however, may be useful in showing the multiplied modes in which lead may be made to act as a poison, and consequently may serve as means of adopting precautions against its deleterious influence. It is probably too extensively used in the different arts. But difficulties in the way of obtaining proper substitutes are so considerable that it seems a hopeless task to attempt either to banish it from use, or to attempt much to limit its applications. Meanwhile it is impossible to doubt that wherever the use of this metal seems likely to be attended with danger, no one but a very rash person would attempt to urge or defend its employment. The most recent proposition that was made, namely, that of employing it in the refinement of sugar, serves merely to show that there is nothing too absurd to be proposed by some persons, and nothing so dangerous that it cannot obtain defenders.

ART. II.—1. *Practical Observations on Midwifery and the Diseases incident to the Puerperal State.* By ALFRED H. M'CLINTOCK, M.D., F.R.C.S.I., Ex-Assistant of the Dublin Lying-in Hospital; Vice-President of the Dublin Obstetric Society; and Lecturer on Midwifery and the Diseases of Women and Children in the School of Medicine, Park Street; and SAMUEL L. HARDY, M.D., F.R.C.S.I., Ex-Assistant of the Dublin Lying-in Hospital, and Vice-President of the Dublin Obstetric Society. Dublin, 1848. 8vo. Pp. 360.

2. *Clinical Midwifery, comprising the Histories of Five Hundred and Forty-Five Cases of Difficult, Preternatural, and Complicated Labour. With Commentaries.* By ROBERT LEE, M.D. F.R.S., Fellow of the Royal College of Physicians, London; Physician to the British Lying-in Hospital and St. Marylebone Infirmary; Lecturer on Midwifery at St George's Hospital. Second Edition. London, 1848. 12mo. Pp. 228.

3. *A Short Sketch of the Life and Writings of the late* JOSEPH CLARKE, M.D., Vice-President of the Royal Irish Academy, and formerly Master of the Dublin Lying-in Hospital, &c. &c., containing minute results of his Private practice, extending over a series of forty-five years; including three thousand eight hundred and seventy-eight births. By ROBERT COLLINS, M.D., President of the King and Queen's College of Physicians in Ireland, formerly Master of the Dublin Lying-in Hospital, &c. &c. London, 1849. 8vo. Pp. 88.

4. *Parturition and the Principles and Practice of Obstetrics.* By W. TYLER SMITH, M.D., London, Lecturer on Obstetrics in

the Hunterian School of Medicine. London. 1849. 12mo. Pp. 395.

5. *On the Theory and Practice of Midwifery.* By Fleetwood Churchill, M.D., M.R.I.A., Honorary Fellow of the College of Physicians in Ireland; corresponding Member of the American National Institute, &c. &c. Illustrated by upwards of one hundred highly finished wood-engravings. Second Edition, corrected and improved. London. Dublin. 1850. 12mo, Pp. 496.
6. *A Theoretical and Practical Treatise on Human Parturition.* By H. Miller, M.D., Professor of Obstetrics and the Diseases of Women and Children in the Medical Department of the University of Louisville. Louisville and London, 1850. 8vo. Pp. 463.
7. *The Principles and Practice of Obstetric Medicine and Surgery, in reference to the Process of Parturition. With One Hundred and Twenty Illustrations on Steel and Wood.* By Francis H. Ramsbotham, M.D., Fellow of the Royal College of Physicians, Consulting Physician on Obstetric Cases to, and Lecturer on Obstetric and Forensic Medicine, at the London Hospital, Physician to the Royal Maternity Charity, Obstetric Physician to the Eastern and Tower Hamlets Dispensary, &c., &c. Third Edition, enlarged. London, 1851. 8vo. Pp. 726.
8. *Cases in Midwifery.* By the late John Green Crosse, M.D., F.R.S. Arranged with an Introduction and Remarks, by Edward Copeman, M.D., F.R.C.S. London, Norwich, 1851. 8vo. Pp. 228.

These works, upon the Principles and Practice of Midwifery, are in different modes and for different reasons well entitled to attention.

Of the first performance on the list, the title scarcely can be said to convey a correct idea of the nature and contents of the work. It is denominated Practical Observations on Midwifery and the Diseases incident to the puerperal state; and it, doubtless, contains many practical observations of great importance, and calculated to be instructive. When to this is added the explanation that it is a Report of the Dublin Lying-in Hospital, and condenses the details of the cases treated in that institution during the three years extending from the 1st of January 1842, to the 1st of January 1845, it must, we presume, be allowed by all competent judges, that if executed with average attention and fidelity, it cannot fail to communicate information most serviceable to those engaged in obstetric practice.

The Report is drawn up by two active and well-informed physicians, who were, at the time referred to, assistants to the master of the Hospital, Dr Johnson. In this respect it resembles the elaborate Report of the same Hospital, published in 1835, by Dr Robert Collins, and of the valuable contents of which an abstract was given in the forty-seventh volume of this Journal in 1837.* The work of Dr Collins, indeed, was the Report for the space of seven years, extending from November 1826; and presented the results obtained from sixteen thousand six-hundred and fifty-four births (16654). The present is not quite so comprehensive; since it embraces not more than the half of the time occupied by the Report of Dr Collins. During the period specified, that is, from January 1st 1842, to January 1st 1845, *six-thousand six-hundred and four women* (6604) were delivered in the Dublin Lying-in Hospital; and gave birth to *six thousand seven hundred and two* (6702) children. From this number of births the authors have, with much industry and fidelity, given in a condensed form, all the details and deductions most important to the science and practice of midwifery.

Of the number of six thousand six hundred and four women, those who were pregnant for the first time amounted to *two thousand one hundred and twenty-five* (2125); which is not much below one third of the whole number. The authors think that this number represents a larger proportion of primiparous females among the parturient than will be found to take place in the population at large. But on this point they speak not positively, as facts are wanting. The authors observe that the circumstance is entitled to attention in estimating and comparing the results.

The total number of deaths that took place within the period specified amounts to sixty-five, thus giving an average mortality of *one* in *one hundred and two*. *Thirty-five* of these deaths took place among primiparous females. The following list shows the causes to which principally these causualties were owing.

Phlebitis and Arthritis,	26	*Anasarca, ...	1
Rupture of Womb,	9	*Scarlet Fever, ...	1
Peritonitis, ...	7	Convulsions, ...	2
Sloughing of Womb & Vagina,	6	Mania, ...	1
Effects of Hemorrhage,	5	*Jaundice, ...	1
*Bronchitis and Pneumonia,	3	*Tumour on Bronchus,	1
*Pulmonary Consumption,	2	*Anomalous Diseases,	2

The asterisk is prefixed to cases in which a non-puerperal disease was the cause of death. The reason for this distinction is the following. There are eleven cases in which a disease not connected with the puerperal state was the cause of death. These, therefore, the authors argue, should be deducted, in order to present a just view of the mortality as necessarily connected with the puerperal state. If, therefore, from the total number of sixty-five eleven be deducted, there are left fifty-four cases,—which reduces the average mortality to one in one hundred and thirty-two. This is all very well. But the authors further express the opinion that puerperal fever should be regarded as an accidental occurrence; and if this be excluded, they add, this reduces the mortality to about one in two hundred and fifty.

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* Edinburgh Medical and Surgical Journal, Volume Forty-seventh, p. 489. 1837.

Upon this point we scarcely feel competent to give an opinion; but we suspect that this mode of estimating the mortality in any given condition of the human race would scarcely be allowed under any other circumstances.

The total number of males born in the hospital during the three years was—

$$3551, \text{ of whom} \begin{cases} 198 \text{ were dead-born.} \\ 62 \text{ — putrid.} \end{cases}$$

The total number of females born amounted to—

$$3151, \text{ of whom} \begin{cases} 127 \text{ were dead-born.} \\ 80 \text{ — putrid.} \end{cases}$$

These numbers give one in thirteen of male children and one in fourteen of female children.

From these numbers the authors derive the inferences:—First, that a greater number of male children die during labour than of female children; and, secondly, that of the children who die in the womb and before commencement of the process of parturition, the number of females preponderates considerably over that of males.

It comes not, however, within the scope of this article to enter into any detailed abstract of the contents of this volume. It is sufficient to mention the subjects discussed, and the order in which they are considered.

First, then, after Introductory Observations, the authors consider the subject of Natural Labour, and introduce their cases of *Uterine Phlebitis*, *Phlegmasia Dolens*, and Puerperal Mania, with appropriate observations and inferences.

Next comes the subject of Tedious and Difficult Labours; their Causes, and the most suitable treatment; with observations on the use of the Vectis, the Forceps, and the Perforator and Crotchet.

In the third division come Preternatural Labours.

In the fourth division Complex Labours come under consideration; and here the authors give an ample and instructive account of the subject of Uterine Hemorrhage, as it takes place before delivery, during the process of delivery, after delivery. Under the same head the subject of retention of the Placenta is considered and illustrated by cases.

The Fifth Division embraces the subject of puerperal Convulsions.

The Sixth Division treats of the important subject of Rupture of the Uterus. Of this frightful accident nine cases are given, all terminating fatally. In eight of these cases the sex of the child was male; in one case only female. All the males were born dead. The female was born alive. The youngest woman in whom the accident took place was twenty-nine years; the oldest thirty-five; all the others were between these two ages.

At the end of this division are given the details of a case in which the symptoms threatened the occurrence of rupture in a woman of thirty-five years. Delivery was effected by lessening the head and then employing the crotchet. The patient came through and left the hospital well on the tenth day.

In the Seventh Division the subject of Twin and Plural births is considered.

In the Eighth Division Presentations of the Chord are examined.

In the Ninth Division the authors communicate the result of their observation and experience in the matter of the Ophthalmia of New Born infants; and the last contains a short code of observations and directions on the Management of Still-Born Children.

Did our limits permit, it would be easy for us to show the valuable character of this Report as an instructive record of practical facts. It may be shortly described as an excellent Treatise on Clinical Midwifery, which will prove most serviceable to the junior practitioners of that art, and cannot be perused without advantage by the most experienced. The facts bear the appearance of having been carefully observed and recorded with fidelity. The qualities now specified are sufficient to recommend the work to extensive use.

To the same class of publications belong the Clinical Midwifery by Dr Robert Lee, and Cases in Midwifery by the late Dr John Green Crosse.

The volume of Dr Lee consists of eight reports upon the most difficult conditions with which the process of Parturition in the human female is liable to be complicated. In each of these reports the author gives an ample and varied list of cases, and makes upon these cases occasional epicritical commentaries explanatory of the principles which they illustrate and the practice which they teach. The lessons which Dr Lee educes from these cases are highly instructive, in inculcating sound and just principles, and they cannot fail to be most serviceable to accoucheurs.

Before quitting this subject, it may be allowed to say, for the benefit of those engaged in the practice of midwifery, that in the

English language there are at least five treatises on clinical midwifery of great value and utility to the practitioner. These are the two Reports of the Dublin Lying-in Hospital, the first by Dr Robert Collins, and the present one by Drs M'Clintock and Hardy; the cases in midwifery, published by Dr John Ramsbotham, first in 1832,* and subsequently in 1842;† the Clinical Midwifery by Dr Robert Lee, and the Cases in Midwifery by the late Dr Green Crosse of Norwich. In these works the reader will find useful information upon all the emergencies and difficulties most likely to take place in midwifery practice.

The Sketch of the Life and Writings of the late Joseph Clarke, M. D., contains an interesting and instructive account of a most industrious and skilful physician-accoucheur. The services of Dr Joseph Clarke in giving precision to many of the departments of obstetrical physiology and medicine, are well known to all interested in the progress of accurate knowledge. In the present memoir, a condensed view of these services is placed before the reader by the nephew of Dr Clarke, namely, Dr Robert Collins. It would be doing justice neither to the subject nor to the author of this memoir, if we attempted more than to mention its nature and object, which is done chiefly to give this performance its proper position in the literature of obstetrical medicine and surgery. At a more convenient season we hope to be able to give more ample attention to a subject of great importance. Meanwhile, when we say that we have read this memoir two several times, our readers will allow that it must be a work of no common interest. It must be generally allowed that Dr Collins has rendered a most useful service by the publication of the interesting facts contained in this sketch.

The other works in the list are more of a systematic character; two of them have been for a considerable time before the profession; and one has acquired a reputation both elevated and extensive.

The work by Dr W. Tyler Smith is remarkable for the originality of its views, its general philosophical character, and the great ingenuity with which the author explains the theory of conception, impregnation, and parturition. One pervading and predominant idea in the work is the doctrine of the Reflex Function of the Nervous System. The application of the theory of reflex action to the explanation of the physiology of impregnation and parturition is the duty which the author undertakes; and it cannot be denied that he is an advocate, both well-informed and ingenious. All the ideas formerly entertained, of the doctrines hitherto taught on impregnation and parturition by Smellie, Hunter, Denman, and many more recent authorities, who need not be named, are, according to Dr Tyler Smith, deficient in not giving natural, just, and philosophical views. These are, he maintains, old obstetrical ideas, (p .28). It is only by recognizing the powerful and unceasing influence of the reflex function of the spinal chord and its nerves, and the mode in which these powers regulate the sensations and movements of the ovaries and the womb, that the true nature, the right action, and the objects of all the movements of the reproductive system are to be found.

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* Edinburgh Medical and Surgical Journal, volume xxxix., p. 453. 1833.

† Ibid. volume lxvii., p. 305. 1847.

There are four forms of Motor action, Dr Smith informs his readers; the Voluntary, the Emotional, the Excito-Motory or Reflex, and the Peristaltic or Immediate.

The womb is not in any degree under the direct influence of Voluntary Motion. It may, however, and often is greatly affected by Emotional action. But the great ruler of all uterine action is the Reflex Function.

The reflex actions of the womb are very numerous; and upon these and upon the multiplied extra-uterine reflex actions excited during the process, essentially depends the natural performance of healthy parturition. Contraction of the womb from irritation of the mammæ, as in the act of suckling the infant; contraction of this organ from the cold water douche applied to the vulva or the surface of the abdomen; contraction excited by irritating the rectum; or by stimulating enemata; or of the stomach by drinking cold water; of the ovaries by the presence of the menstrual nisus; or the vagina by manual irritation; of the *os uteri* by introducing the hand into the uterus,—are all to be considered as instances of reflex spinal action. (P. 35.)

Parturition is not one reflex action, but a function, the combined result of many such actions, aided by other powers. The womb, as a motor organ, stands alone in many respects. (P. 37.)

The Peristaltic is another action belonging to the uterus. It consists in a motion spreading in a vermicular manner, from the point of irritation, and continuing for some time after the irritation has ceased. With this form of contraction the womb is eminently endowed. When one part of the womb is stimulated through the abdominal parietes, the contraction excited extends with the utmost rapidity to the whole organ. The same occurs when the fingers irritate any point of the internal surface of the organ. To this action Dr Smith refers those cases in which, on occasion of the mother dying suddenly or having been drowned, an infant had been found expelled lying between the limbs. By means of the same peristaltic action, he explains a phenomenon something singular, namely, that of inversion of the womb after death in cases also of sudden death.

The whole of this third lecture may be recommended as highly ingenious and instructive.

In the fourth lecture the author explains, upon the principle of reflex action, his views on the sensibility and movements of the Fallopian tubes, in menstruation, sexual intercourse, conception, and parturition.

In the fifth lecture an instructive account is given of the influence of the nerves of the womb and ovaries on their different functions. Dr Tyler Smith adopts the side of Dr Robert Lee, and renders justice to the labours of that gentleman in demonstrating the existence of the uterine and ovarian nerves, and showing their influence upon the functions.

Much has been recently said upon the influence of the ovaries in menstruation and other uterine conditions, upon the ovular theory of menstruation, and, in short, upon the power exerted by the ovaries in maintaining all the uterine and reproductive actions in a state of health and activity. These subjects are explained in a very clear and instructive manner by the present author, who has taken great pains to show the exact amount of evidence, upon which the doctrines may be said to rest.

In speaking of the ovular theory, he renders justice to the physician who first invested that theory with the importance which it at present posseses, and his account of the comparative obscurity and unobtrusive silence in which Dr Power has spent much of his life, is only too correct a picture of the condition of many able men in a profession, in which few are competent judges of real merit, and the bustling and active very commonly get the better of those of independent minds.

"The Ovular theory of menstruation was next revived, or rather originated, by Dr John Power, who made it the subject of one of his Essays on the Female Economy, published in 1821. By an intuitive sagacity, based on many original observations of his own, and on the facts and opinions of his predecessors, he arrived at the conclusion, then stated distinctly for the first time, that at every menstrual period an ovule reaches maturity in the ovarium, and is discharged from it during the flow of the catamenia. He taught that the catamenial secretion was excited in the uterus by the condition of the ovaria, and that the menstrual fluid was an imperfect attempt at the formation of the decidua; the ovarian stimulus when the ovule is unimpregnated, not being sufficient in ordinary cases to excite the deposition of fibrin, but stopping short at the menstruous secretion. He, however, drew attention to the fact, that in certain cases of dysmenorrhœa the ovarian stimulus is greater than usual, and that then the false membrane of dysmenorrhœa may be formed. He looked on the uterine phenomena of the menstrual function as an abortive attempt at the formation of a deciduous membrane, preparatory to the reception of the abortive ovule. So far, there is little to add to the theory of menstruation at the present time. I should mention to you that Dr Power does not seem to have been aware of the observation of Cruickshank or Kerkringius. His essay contains the most striking evidence of an original genius. It must, however, be said, that the ova of Dr Power were almost as hypothetical as those of Kerkringius. He knew of no ova except the vesicles of Kerkringius and De Graaf, and he drew a wrong analogy in comparing the formation and expulsion of the membranous secretion in dysmenorrhœa, to the extrusion of the unimpregnated egg of the pullet; and while he recognised the analogy between the uterus, as the nidus of the human ovum, and the nest of the bird, he appears to have failed to see any analogy between œstruation and menstruation. With these drawbacks, which are not very important, considering the state of physiology twenty-five years ago, the classical essay of Dr Power is quite on a par with the last word of science, and it affords a very remarkable and beautiful instance of the advance of theory before fact, as we shall immediately see. His mind saw the inevitable necessity for the existence of ova, and he wrote as though he had actually observed them."—Pp. 79, 80.

"The theory of mensturation was very much ridiculed when first propounded by Dr Power. Its author was treated as a visionary, and I have heard he acquired among accoucheurs the name of 'Menstruation-Power.' His little book was severely scourged by the critics of that time; but the event is another proof, that whenever ideas of sterling worth are put forth, the world, even if it do not at once appreciate their value, is sure to find them 'after many days.' Dr Power himself seems to have been little careful about the result; he never appears to have discussed the matter, or even resorted to experiments; but to have calmly thrown his ignot of gold into the stream of time, there to be refined, instead of casting it into the burning fiery furnace of controversy, for that more rapid assay which original minds sometimes covet. Very recently, I paid a visit to him, at his residence at Westminster. and I am proud to say he was much interested in my own researches in the physiology of Parturition. I found him amusing himself with painting and composition. He showed me some geological landscapes, if I may so term them, from his pencil, the still-life and figures of which were composed from the study of geological strata and fossil remains; icthyosauri, plesiosauri, and other monsters basked, or pursued their prey, in scenes belonging to a primeval world. I mention this to show the original turn of the mind to which we owe the true theory of menstruation. It is little to the credit of our profession or of science that he has never received any reward whatever for his beautiful discovery. Indeed, until recently, it was not known whether he was alive or dead. He has neither courted nor received distinctions and honours of learned societies, British or foreign; born of a family in whom medicine may be almost said to be hereditary, and of which, until lately, three generations of physicians were living, he has still no other title or distinction save the M.D. of his professional diploma."—Pp. 80–83.

This neglect of the merits and talents of Dr Power we may lament; but it is altogether impossible to prevent or amend such results. In a country like this, where utility, real or imagined, is the great test of the excellence of every thing, whether in science or art, and especially where the desire to obtain money is so universal, it is altogether idle and chimerical to expect, that either science or any other thing shall receive its reward, unless in so far as it is capable of being the means of gaining money.

"In treating of menstruation, I have attempted to keep your minds fixed on the ovaria as the theatre of the essential phenomena of this function, and because their actions have an intimate relation to parturition. The secretion from the internal surface of the uterus is purely secondary. I do not believe that there is a single fact on record to shew, that the catamenia are secreted from the uterus in the absence of the ovaria. Cases in which this has been supposed to happen are probably not truly menstrual, but similar to the hæmorrhoidal discharges in the male, which are sometimes periodic. But there are recorded instances in which all the pains and excitement of menstruation have occurred regularly in women, in whom the ovaria have been present, and in whom cicatrices have been found upon the ovaria, but with the uterus entirely wanting. I firmly adhere to the opinion of Dr Power, that the catamenial secretion or exudation is an abortive attempt to place the uterus in a position to receive and attach the ovum to its cavity. When Dr Power wrote, the nature of the menstrual secretion was not understood, its identity with the blood was disputed, and especially it was said that this fluid contained no coagulable matter. But some recent observations of Mr Whitehead, of Manchester, are very interesting upon this point."—P. 86.

"Having convinced ourselves that the ovaria are the organs primarily concerned in menstruation—that the catamenia are the effect, and not the essential part of this function—we shall be short-sighted, if we do not at once refer many of the disorders of menstruation, not to the uterus, but to the ovaria. I know of few applications of the modern physiology of menstruation to practical medicine. One of the most important is the plan of treatment recommended by Professor Naegelé is sterility. He prescribes, that in certain cases coitus should take place during the presence of the catamenia. In many other cases the ovular theory of menstruation suggests that the greatest attention should be paid to women affected with sterility immediately after the cessation of each catamenial flow. In the middle of the interval between the periods, there is little chance of impregnation taking place. The same kind of knowledge is of use, by way of caution, to women who menstruate during lactation, in whom there is a great aptitude to conceive; pregnancy, under such circumstances, would be injurious to the health of the fœtus, the child at the breast, and the mother herself, and therefore should be avoided, if possible. Another application of this theory has been in the more satisfactory determination of the duration of pregnancy, and the explanation of those cases in which pregnancy is prolonged beyond the tenth catamenial period.

" Amenorrhœa and amenorrhœal sterility are clearly affections not of the uterus but of the ovaria. When we give emmenagogues empirically, with a view merely to excite the menstrual flow, we place medicine far in the rear of physiology. The essential cause of amenorrhœa is in the absence of the periodic ovarian excitement, and the maturation of ova. We can only cure amenorrhœa by bringing the ovaria into that condition which admits of their periodic excitement and the extrusion of ova, and this we must do either by local or general stimuli, or both. If in a case of amenorrhœa we paint the inner surface of the uterus with nitrate of silver, as some have recommended; or inject a solution of ammonia, and so produce the uterine secretion, we do not cure the amenorrhœa, unless, indeed, these agents affect the ovaria at the same time; we, in fact, only remove a *symptom*; for the amenorrhœa, the absence of the menstrual flow, is a symptom, and not the whole disease. We want a new term, which shall include both the ovarian and uterine conditions of this disorder.

" In dysmenorrhœa, or painful menstruation, the greater portion of the pain consists, I am convinced, of ovarialgia; the deep lumbar pain is decidedly ovarian, and not uterine. Many women suffer so much lumbar pain at each menstrual period, that it resembles, and indeed, almost amounts to, a monthly attack of ovaritis. Almost all women in the better classes suffer so much pain and disturbance from menstruating, that we may almost venture to say menstruation, like parturition, lies in debateable ground, between physiology and pathology; but of this more hereafter. Part of the pain of dysmenorrhœa, then, is ovarian, and that which is uterine is often symptomatic of ovarian disorder. In dysmenorrhœa, there is doubtless a pathological state of the uterus induced; but there would be no uterine excitement without the previous excitement of the ovaria. On the other hand, there are patients in whom the uterus is wanting, from congenital deficiency, who suffer all the ovarian pain of dysmenorrhœa. Uterine disturbance must be considered as a secondary condition—an aggravated symptom of ovarian excitement in painful menstruation."—Pp. 87-89.

" I am decidedly opposed to the view that dysmenorrhœa is *caused*, in the majority of cases, by chronic inflammation of the *os* and *cervix uteri*. The relation between dysmenorrhœa and inflammation of the *os uteri* is generally one of coincidence, not of causation; nay, it is often a symptom induced by ovarian irritation, a symptom requiring palliation, but the relief of which by no means constitutes the whole of our treatment. Let any one who believes in the merely uterine theory of dysmenorrhœa, closely examine the nature and seat of the pain; he will speedily be obliged to recognise the paramount influence of the ovaria. Of course, wherever there is inflammatory action of the *os* and *cervix*, the inflammation will be rekindled at every menstrual period, and constitute a variety of painful menstruation; what I am contending against is, the too exclusive attention of the practitioner to this superficial form of dysmenorrhœa.

But the most serious disorders of menstruation are the hysteric and epileptic convulsions, which are sometimes excited by the ovarian irritation. The first attack of epilepsy frequently invades women at the coming on of a catamenial period; young girls are liable to convulsive attacks on the first appearance of the catamenia; and in confirmed epileptics the fits are always most violent and prolonged during the accession of the periods. *Ovarian irritation is, I am persuaded, the most important of all the causes of epilepsy, in the female.* Ovarian irritation is also a fruitful source of hysteric convulsion, and there is no other state of the economy in which the fits of hysteria and epilepsy run so much one into the other, and where the diagnosis requires to be made with equal care. In these cases it is not so much the uterine as the ovarian excitement which is the cause of the convulsion. It often happens that the fits of epilepsy occur before the commencement of the uterine secretion, when there is little uterine disturbance, and that they cease immediately on its appearance, the secretion from the uterus effectively depleting the neighbouring ovaria.

The influence of the ovaria upon the intellect and the emotions, are as remarkable in diseased as in healthy states. At each catamenial period the temper is disturbed in women of irritable constitution; in some women almost to madness. Indeed the ovaria appear to be an exciting cause of insanity in unmarried females, in the puerperal state, and at the catamenial climacteric. At the catamenial climacteric a revolution of the emotions commences, which requires especial study. General hyperæsthesia and hyperemotion exist at this epoch, of the most distressing character, both to the patient and her friends. Not the least singular feature of mental disturbance from ovarian irritation, is the sudden appearance and subsidence of the disorder. The most violent mania may appear almost without premonition, and disappear as abruptly. But I can only allude to these subjects, and to the influence of the ovaria in chlorosis, anæmia, hysteria, and various other affections peculiar to the sex.

In all such affections the treatment must be, not merely that which removes uterine disorder, but that which relieves undue ovarian excitement and irritation, both in the intervals between the periods and during the periods themselves. In the pathology as well as in the physiology of menstruation, the first place must be given to the ovaria. Their pathological synergies are quite as decided as those of a physiological kind."—Pp. 89–91.

In Lecture Eighth Dr Tyler Smith applies the doctrine of reflex action to the movements of the womb during parturition. The lecture is ingenious and cannot fail to be perused with interest. The following passage is all that we can make room for.

"The duration of human pregnancy is well known to be a multiple of a catamenial period. It has been also observed, that in the rare cases, where the duration of pregnancy exceeds the ten menstrual periods, the function of parturition is deferred to the following period, so as to make pregnancy reach to eleven periods inclusive. This fact came out most clearly in the obstetric evidence on the Gardner peerage case, and will prove of very great importance in cases of supposed criminality in wedlock, or in establishing the legitimacy of posthumous children. On the other hand, it is well known, that when utero-gestation is brought to a premature termination, it is at what would have been a menstrual period that abortion usually takes place. There is, in fact, in all women, a tendency to abortion at the times represented by the catamenial periods. In placenta prævia, the hæmorrhage occurring in the later months of pregnancy generally happens at the periodic dates. Hæmorrhage sometimes occurs for two or three periods in succession before parturition in these cases, with perfect immunity in the intervals. The occurrence of uterine pains at the termination of the natural time of gestation, in cases of extra uterine fœtation, points very decidedly to the operation of some extra-uterine source of excitement. After abortion,—and very speedily, too, in some cases,—it is known that there is a remarkable aptitude to conceive; and during the catamenial climacteric, when the ovaria are irregularly excited, there is frequently an increased tendency both to conception and to abortion. These accumulated facts are sufficient to establish the influence of the ovaria and of the ovarian function on the uterine phenomena of gestation and parturition."—Pp. 115, 116.

Dr Tyler Smith is opposed to the employment of anæsthetics. Besides a pretty decided protest against their employment at page 117, immediately posterior to the above extract, the following observations are given at page 168 in the Eleventh Lecture.

"A year ago, and all women were promised safe and perfect exemption from the pains of natural labour. Now, however, the anæsthetic Elixir proves to be but a cup of Circe, instead of the true birth-help. At the present time, when the febrile excitement of the medical mind upon this subject is rapidly passing away under the influence of the numerous fatal cases which have occurred from the use of anæsthetic agents, there seems a more than ordinary necessity for dwelling upon the manifold wisdom with which the act of parturition is surrounded. Woman will certainly derive truer comfort, and a greater measure of safety and freedom from unnecessary suffering, from physiology, than from a wild therapeutics, which, in her hour of trial, only offers a choice betwixt poison and pain."—P. 168.

The only other passage for which room can be made is the following upon Ovarian Irritation.

"Ovarian irritation is one important cause of precipitate labour and undue uterine action. The ovaria are the *vis à tergo* of parturition. In some cases, the ovarian stimulus, which in its normal degree produces the proper, physiological excitability of the uterus, on which natural labour depends, is excessive, and induces in the spinal centre and the utero-spinal nerves a state of excitability almost of a tetanic kind. All the motor actions of labour are immoderately increased. The whole surface of the parturient canal becomes intensely excitor, and first the liquor amnii, then the head and trunk of the fœtus, and the manipulations of the accoucheur, produce the most rapid and powerful contractions, so that every uterine pain amounts to a local convulsion. Unless the nervous energy becomes exhausted by the uterine actions of the earlier stages of labour, the excitability increases with the progress of parturition, and reaches its acme at the time of delivery. The physical pain is in proportion to the motor action, being increased by the great pressure exerted on the uterine nerves, and by the sudden and forcible impulsion of the presentation through the passages; in such cases, the morbid uterine motor action, and the injury done to the parturient canal, may be said to partake, the one of the nature of cramp, and the other of violent contusion.

"Though the nerves of the ovaria are the primary excitors of the neural energy exhibited in parturition, there are other organs, independent of both uterus and ovaria, which exalt the excitability of the spinal centre and the utero-spinal nerves. Thus a loaded state of the rectum, excitability of the bladder, and certain states of the stomach, act upon the uterus, and produce excessive action of the latter organ. The most important of these sources of excitation is the rectum, which, when loaded, will sometimes derange the whole function of labour, rendering the contractile pains quite uncontrollable. During labour the uterus attracts, as it were, to itself, the reflex effects of local irritation in any part of the body."—Pp. 216–217.

Irritation indeed of the ovaries, all good observers will allow to be a much more frequent condition than inflammation; and we cannot refrain from repeating the opinion expressed in last volume, when speaking of the hypothesis brought forward by Dr Tilt, that the pain which is felt at menstrual and other periods, though acute, is indicative not of inflammation but of irritation. The existence of inflammation as a condition into which the ovaries may fall we do not deny. It is a possible, perhaps not an unfrequent occurrence. All that we contend for is, that it is neither philosophical nor consistent with observation to ascribe to inflammation, all those conditions in the female organs which are attended with pain even of considerable severity.

We observe that Dr Fleetwood Churchill signifies his approbation of the opinion of Dr Tyler Smith, that the chief seat of neuralgic dysmenorrhœa is in the ovaries; (p. 60).

Before quitting this subject, we may be permitted to say, that among the writings of physicians and accoucheurs, ample evidence may be found to show, that painful mensuration, though it might seem to suggest to the minds of many inflammatory action, either in the ovaries or in the Fallopian Tubes, nevertheless, was generally believed to indicate the presence of that species of disorder which is now called irritation, neuralgia, exalted innervation. Many authorities might be adduced to prove the statement now made. But we shall go no farther than to Dr John Fothergill, an eminent physician in London during the middle of the eighteenth century, and one of the great supporters of that Association of physicians and surgeons, to whom we not long ago said, the profession was indebted for the collection entitled the Medical Observations and Inquiries.

" Nothing, perhaps," says this physician, " is more excruciating to the patients, nor, in common, more difficult to cure than painful menstruation. It impairs their health at present, and seems to render them less prolific in future. To the sufferers it is a most serious evil." " This excruciating pain seems to be spasmodic, and to proceed from the extreme irritability of the uterine system. The blood naturally determined hither, in order to its being discharged, by distending the very irritable vessels, occasions the spasm; this produces a constriction of the vessels; they become impervious, and the *nisus* to the discharge continuing, the pain becomes exquisite and general, till the patient, worn out with the struggle, is debilitated and sunk. The glands are then dismissed; some case suceeds; but the patient is often so reduced as not to recover her usual strength before she has another conflict to undergo."

" The *Fluor albus* is frequently the consequence of this struggle; and it would seem as if the *Uterus* itself was so far a sufferer as to be rendered, by degrees, less fit for fecundation. I think it has been observed by other physicians as well as myself, that few of those who have suffered much in the manner here described have borne children."*

Now, upon the hypothesis here expressed, any opinion may be entertained that readers think proper. But it is quite clear that here is the doctrine of irritation as distinctly expressed as can well be done. Even the expression *irritability of the uterine system* may be taken without difficulty to imply the irritable state not only of the uterus, but of the ovaries and Fallopian tubes. It is also a well-known fact that dysmenorrhoea and leucorrhoea, in a great many cases are conjoined, and that the latter is not cured until the irritable state of the uterine system has been removed.

The author proceeds in succeeding lectures to apply the doctrine of the Reflex Function to explain the phenomena of Rupture of the Womb, and Laceration of the Perinæum (lecture sixteenth); Inversion of the uterus, After Pains (lecture seventeenth); and Puerperal Convulsions (lecture twentieth, twenty-first, and twenty-second.) This latter portion may be said to be the most elaborate of the work; and it certainly presents a very plausible view of the physiological and pathological relations of convulsions in the puerperal state.

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* On the Management Proper at the Cessation of the Menses. By John Fothergill, M.D., F.R.S., Medical Observations and Inquiries, VolumeV., Art. XVII. 185. London, 1776,

It would not be difficult, did our limits permit, to make from this small and ably written volume many extracts of interest. But enough has been said to show the general character of the work, and to induce, we trust, not merely obstetrical, but medical readers in general to peruse it. Whoever peruses it impartially and patiently will, we think, arrive at the conclusion that the author is a zealous and able advocate for the Reflex Function, and that he evinces a very considerable degree of address and skill in its application to the phenomena, normal and abnormal, of impregnation and parturition.

In giving this character of the work of Dr Tyler Smith, we refrain from expressing any opinion as to the truth or the well-foundedness of what has been called the doctrine of the Reflex Function. The facts which have been referred to this function, and which are believed to prove its existence, have been long well-known. These facts are probably most easily remembered and most readily understood by referring them to this head. But it must not be forgotten, that it is a name which is employed merely in reference to the mental faculties; and that after all, it is an attempt to explain by a mechanical act, and by comparison with mechanical acts, phenomena which are not thereby made in any degree more distinct, and which are necessarily of a nature so obscure, that though their effects are seen, their actual operation entirely eludes sensible observation of every kind.

5. The Theory and Practice of Midwifery by Dr Fleetwood Churchill is a treatise composed with great skill and labour, and containing an immense amount of useful instruction. Giving very correct views of all the facts and principles which require to be known to the scientific accoucheur, Dr Churchill has further greatly enhanced the value of his work by the ample collection of statistical facts and tabular results with which he has provided the student.

It is well known, that in the Principles and Practice of Midwifery considerable difference of opinion as to certain measures to be followed in particular cases, has been long entertained by the ablest and most intelligent practitioners. It is very likely that this difference in opinion will continue so long as Midwifery is a science; for there are in certain divisions of the subject inherent and insuperable difficulties which it may not be easy to remove. One of the most probable means of diminishing, if not removing and overcoming these difficulties, is to make extensive collections of facts, and from these to endeavour to educe fair average results. Though the knowledge of these averages by no means removes the difficulty and danger for particular cases and emergencies, it makes the nearest approach that is practicable to doing so. It applies the doctrine of numerical averages to determine the probable results. It is always, nevertheless, a point to be ascertained, under which category the favourable or the unfavourable, the particular case is eventually to place itself. To this department of the subject Dr Churchill has devoted much labour and attention, and he has been at great pains to show the reader the evidence, by which he is to be guided in practical proceedings.

In this way Dr Churchill has studied to determine not only the most important varieties and difficulties in the process of parturition, but the applicability of all the great operations, and the results which they may be expected to afford.

6. The sixth work on the list is, as our readers may perceive, the production of an American United States Professor in the far West.

Dr Miller has been desirous to discover, as are most authors, satisfactory reasons for publication. He allows that there are numerous works on Midwifery already before the profession ; and though he admits that all have excellencies, yet he adds that all have faults. "While they have disseminated knowledge and enlightened the highways and byways of practice, and thus been instrumental in achieving much good, they have unsettled the minds of practitioners in regard to points of deepest interest, and made a wreck of obstetrical nomenclature, as far as the presentations and positions of the fœtus are concerned."

Two serious charges are here brought against all the writers that have hitherto attempted to instruct the rising generation of accoucheurs. Dr Miller accordingly undertakes, in the present performance, to clear away all doubts and perplexities that have been engendered, he maintains, as to matters of fact. He is to give a full, correct, and lucid description of the mechanism of labour; this being the leading object which in composing his book he had in view.

The fœtal presentations and positions, Dr Miller maintains, have been thrown into confusion and disorder, the denominations and language employed are not understood. In most countries, he says, there is, and in all countries there ought to be, some standard agreed upon relative to this part of obstetrical medicine; but how, he asks, can such agreement exist in a country like ours which tamely consents to receive its science as well as its fashions from abroad? The United States need, he concludes, "at this time a native authority to educe order out of confusion, and set up a national standard, under which all our practitioners may arrange themselves and be cemented by a common bond of union."

Dr Miller accordingly avows, that to review the whole subject of presentations and positions with the light which his own experience and that of others has shed upon it, since the time of Baudelocque, and establish for them a classification and nomenclature which it seems to him all may adopt, is another object which the author has in view in offering this volume to the profession. The classification which he recommends is that of M. Dugés; but the nomenclature is his own.

The author seems to think that persons on this side of the Atlantic may blame him for presumption, and treat with disregard or even ridicule the attempt of a backwoodsman to erect a standard. This at all events we shall not do. We daresay Dr Miller sincerely believes that another book on midwifery is necessary. As to such remarks as that of countries consenting to receive their science, as well as fashions from abroad, we fear that the confusion of ideas, of which this mode of speaking is an example, is not altogether confined to the doctrines and distinctions of obstetrical writers. A little reflection should teach Dr Miller, that fashions are one thing and science is another altogether different. There may be an English fashion, a French fashion, and a United States fashion. But science is neither English, French, nor United States property. It belongs to the world at large, and never can belong to any individual country. To talk, therefore, of receiving science from a country as fashions might be received and are received, makes a near approach to some thing not to be named in these pages.

In other words, all that Dr Miller means to say is, that upon difficult and doubtful questions in obstetrical art, different individuals will entertain different views; and it is both natural and right that dissentients should take means to make known what they conceive to be correct, and point out what they think to be erroneous.

The best reason for publishing a book is, after all, that the author was determined to write and to publish. The validity of this as of all the rest, must be left to be determined by his readers.

The work of Dr Miller is a very good book, and shows that the author is well acquainted with his subject. He has truly bestowed great attention upon the subject of the presentations and positions of the foetus. The work is divided into twenty-four chapters; and of these the examination and history of the presentations occupies ten; namely, from the beginning of the thirteenth to the end of the twenty-second chapter.

Willingly would we endeavour to give an abstract of this part of the author's treatise, but our limits prevent us from so doing. To communicate, however, some idea of the manner in which Dr Miller treats his subject, and examines conflicting evidence upon doubtful points, we subjoin the following extract upon thè treatment of the first stage of labour.

"The first is the most dilatory of all the stages of labour,—it being the preparation for what is to follow, and preparation, in most things, usually requiring a longer time than execution. It may, therefore, be reckoned a prime duty of the accoucheur to wait patiently, as a general rule, until this stage is naturally accomplished. But is there no limit to the patience he must exercise? Is there no dilatoriness on his part which is reprehensible? Must the first stage of labor be always left to take its course, unless there be some uncommon and palpable necessity for interference?

"These are important questions, and their solution, more than anything else in obstetrics, settles the practical complexion of each individual to whom they are propounded. Hear the answer of Dr Denman;—'*Whether a short or a long time* be required for this purpose (the dilatation of the *os uteri*), it is the duty of the practitioner to abstain from interfering in this part of the process. It may sometimes be necessary to pretend to assist, with the intention of giving confidence to the patient, or composing her mind. But all artificial interposition contributes to retard the event so impatiently expected, by changing the nature of the irritation and the action thereon depending, or does mischief by inflaming the parts, and rendering them less disposed to dilate; in short, by occasioning either present disorder or future disease.' * So spoke the British oracle, in the latter part of the last century; and, with two or three exceptions, his successors to the present time, have echoed the authoritative response. The published lectures of one of them, who is yet living;† are full of kindred advice, eloquently and vehemently enforced,—exhorting the pupil to abstain from every species of interference, unless urged by dire necessity; and turn where he may, almost upon every page, the apothegm stares him in the face,—'a meddlesome midwifery is bad.' The deepest impression which his teaching is calculated to make, is, that nature is particularly careful not to be at fault in the matter of childbirth, and that it is a fearful thing to do aught but admire her proceedings.

"I have said that one's cast as a practitioner is determined by his views of professional duty, in the management of the first stage of labour; if he perform the part of an *expectant* in the first act of the drama, it is not likely that he will appear in any other character in the subsequent acts. Now, it is notorious that Dr Denman, for example, is hesitating and indecisive in the advice he gives, touching the conduct of the obstetrician in every branch of his duties; and after most carefully considering his views, one can hardly determine whether, upon the whole, he had best to do something or nothing.

"With the tenets of Dr Denman contrast those of the late Professor Hamilton, of Edinburgh, who states that he was but a very short space of time in practice, when he saw reason to believe that the management of the first stage had been much misunderstood by the profession. Observing that when the natural powers alone are trusted to, this stage is often greatly protracted, he inferred, from reasoning upon the subject, that injurious effects must be the consequence. 'He considered,' to use his own language, 'that upon the occurrence of every uterine contraction, there must be a certain influence on the action of the heart and arteries, and that if pain and sleeplessness were continued beyond a limited time, there must be an exhaustion of sensorial power. He concluded, therefore, that where the first stage of labor is not completed within a certain time, the strength of the patient must be proportionably lessened, the uterine action must be enfeebled, and the circulation of the blood must be disturbed.'† But, finding that the most respectable practitioners, both British and foreign, deprecated all interference with the first stage of labor, he felt diffident in promulgating his opinions, and did not do so until the year 1800, when he stated as the result of his observation and experience, for about fifteen years, that 'unless the first stage of labor (supposing that there are regular pains) be completed *within twelve or fourteen hours* from its real commencement, the following consequences may be dreaded.

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* Introduction to Midwifery, chap. ix. sec. 6.

† Lectures on the Principles and Practice of Midwifery. By James Blundell, M. D., edited by Charles Severn, M. D. Philadelphia, 1842.

" 'Firstly. That the powers of the uterus may be inadequate to expel the infant with safety to its life, or to the future health of the parent.

" 'Secondly. That after the birth of the infant, the uterus may contract irregularly, so as to occasion the retention of the placenta.

" 'Thirdly. That after the expulsion of the placenta, the contractions of the uterus may be too feeble to prevent fatal hemorrhage. And,

" 'Lastly. That, supposing the patient should escape all those untoward circumstances, febrile or inflammatory affections of a most dangerous nature may ensue, from the previous protraction of pain and the irregular distribution of the blood.'

" In view of these facts, Dr Hamilton adopted it as a rule of practice, that *the termination of the first stage of labor should be secured within twelve or fourteen hours from its actual commencement.*

" In prescribing limits to the first stage of labor, he is careful to guard against any mistake or abuse that might grow out of the restriction. There must be a continuance of regular pains for the period specified, 'for it sometimes happens that, after regular pains have commenced, the agitation of the patient, or the mismanagement of the attendants, occasions a suspension for some hours. If there be no injurious pressure upon the passages during that suspension, the patient's strength is recruited, aud the duration of the first stage is to be reckoned from the recurrence of the pains.' And then again, spurious pains are to be discarded from the estimate; these may precede the true ones for hours, or days, producing no tightening of the edges of the *os uteri;* and unless this is present, labour has not really commenced.

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* Practical Observations on various subjects relating to Midwifery. American edit. part i. p. 60.

"By the adoption of this rule, the author asserts confidently that '*no patient under his charge, for the last thirty-five years, has been above twenty-four hours in labour, and, except in cases of disproportion, none so long.*'

"Such a result as this,—so encouraging to those who are expected to alleviate the sufferings, and abridge the anguish of the sex, and who derive unfeigned pleasure from the fulfillment of their responsible mission,—challenges the candid consideration of every one, who is qualifying himself for the practice of obstetrics. It is entitled to the sober second thoughts of such as are already engaged in practice under a different creed; but, alas! with how little prospect of success can they be appealed to, since the principles, from which this result was obtained are condemned as heretical by nearly all authors, except Professor Burns. The latter even abridges the period prescribed by Dr Hamilton, within which the first stage of labour should be completed. 'If,' he says, 'the pains be continuing without suspension, for an interval of some hours, and the labour be going on all the time, but slowly, it is a good general rule to effect the dilatation of the *os uteri* within *ten* or *twelve hours* at the farthest, from the commencement of regular labour.'* In relation to the necessity of this he speaks thus confidently. 'It is an undeniable proposition, that there is in every case a period beyond which it cannot be protracted without exhaustion; and it is no less certain, that if we wish to avoid this exhaustion, which may be followed by pernicious effects, we have only the choice of either suspending the action altogether for a time, or of endeavouring to render it more efficient, and of effecting the desired object within a safe period.'

"The doctrine of Hamilton and Burns, on this subject, I have ever considered sound in the main, and their practice worthy of imitation. It is, therefore, incumbent on me to explain the one a little more fully, and defend the other. The doctrine assumes that the uterus is incapable of continuing its parturient action beyond a definite period, notwithstanding its frequent respites, without falling into a state of exhaustion that unfits it for the proper performance of its function. And does any one doubt the truth of this assumption? If so, let him refer to any treatise on practical midwifery, worthy of its title, in which there is not a distinct recognition of a *powerless state* of labour,—not that the uterus is so completely exhausted as to cease its action altogether, but it is so enfeebled as to be disqualified for efficient action, and the delay and danger, incident to such a state, call loudly for assistance. It is worthy of observation that before the uterus reaches this deplorable state, it is liable to become morbidly irritable and sensitive, so that it is disposed hastily to eject its contents, the instant the obstacle is removed, which provoked this morbid state. This irritable condition, as I may have occasion to show, is scarcely less dangerous than the exhaustion of which it is the precursor.

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* Principles of Midwifery, American edition, with Notes by Professor James, vol. i. p. 417.

" But although it may be conceded that the uterus is liable to exhaustion from long-continued exertions, it may be denied, as in fact it has been by Dr Churchill,* that the pains of the first stage have any such pernicious tendency. In his chapter on 'Tedious Labour, Dr Churchill lays down the following propositions: '(1.) When the delay is excessive, the *relative* duration of the two stages is destroyed, so that they bear no steady proportion to each other; thus, for instance in a labour of sixty hours, the first stage may occupy fifty-nine, the second only one, or *vice versa;* (2.) That the effects of a prolonged labour upon the constitution of the patient, depend upon the stage in which the delay occurs; and (3.) That delay in the first stage involves very little if any danger, no matter how tedious it may be, but that delay in the second stage, beyond a comparatively short time, is always serious of import.

" These deductions, Dr Churchill thinks, are involved, though not distinctly enunciated, in the practical remarks of writers on midwifery, who distinguish the cause of delay in the first stage from those in the second, as being much less, dangerous: and in further corroboration of them, he publishes a tabular synopsis of one hundred and forty-three cases, to exhibit the relative duration of each stage, in labours of twenty-four hours and upward, in which the delay occurred in the first stage, and the results to mother and child. The table offers such cases as the following;—first stage 34½ hours, second half hour,—first stage 41¾ hours, second quarter hour;—first stage 59½ hours, second half hour,—first stage 176 hours, second 1 hour. Notwithstanding the tediousness of the labours, all the mothers recovered, and but ten of the children were lost, one of which was putrid. It is plain that these statistics, supposing them to be accurate, are fatal to the views of Professor Hamilton; for it would appear from them that the powers of the uterus are not enfeebled in the slightest degree, by the utmost prolongation of the first stage; on the contrary, the second stage is, notwithstanding, executed with remarkable facility, and without involving the least risk, present or prospective, to the mother; the only disadvantage being the loss of a larger proportion of the children, than in labours performed in better time.

I was, I confess, greatly astounded when my attention was first directed to these statistics; not because they militated against a favourite doctrine or theory (that I could have easily surrendered at the summons of truth), but because if they are to be relied on, I have yet to learn the alphabet of practical midwifery. Experience for more than a quarter of a century (a long time to be under delusion), has seemed to teach me that if the first stage be unusually protracted, the second is *liable* to be so too; or where it is not correspondingly delayed, it is *liable* to be executed so precipitately as to endanger both mother and child,—the uterus having acquired the morbid irritability to which I have alluded. In the latter case, irregular contraction is apt to follow the expulsion of the infant, producing difficulty in the third stage.

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* Theory and Practice of Midwifery. Philadelphia, 1846.

"In rehearsing my experience, in general terms, I have been careful to say 'liable,' because there is no rule without exceptions, and it does sometimes happen that tedious first stage is followed by safe and reasonable promptitude of the second. These exceptions are not, however, numerous enough to account for Dr Churchill's one hundred and forty-three cases, which he says expressly were not culled for the occasion. How, then, are they to be accounted for? I could never have answered the question, if, in turning over his pages, I had not stumbled on his definition of the first stage, which is as follows:—'extending from the commencement of labour to *the passage of the head through the os uteri*" (p. 199). When the head has passed through the os uteri, it is, I opine, near its journey's end; for nothing is more common than for the anterior edge of the os uteri to be felt in advance of the head, a few moments before it emerges, although the pains may have been decidedly expulsive, that is, the second stage existed, for hours previously.

"The complete dilatation of the os uteri, spoken of as the end of the first stage, is never its obliteration, so that it cannot be felt and does not even require a degree of force to engage the head in it,—but such an opening of it as will allow the head to pass with no more resistance than the other soft parts, the vagina, perineum, and vulva, ordinarily offer,—for labour is a struggle throughout. Before the os uteri is dilated to this degree, the membranes protrude through it, and with *sufficient* dilatation of the orifice and the *formation* of the membranous pouch, the first stage of labour closes, and the second is ushered in by *expulsive* contraction, only slightly different at first from the *propelling* contractions of the former.

"According to my understanding of what belongs to the several stages of labour, then (which accords with the best writers), Dr Churchill's second stage is indebted for its remarkable brevity to his gratuitous bestowal of a goodly portion of it upon the first stage, and his table is, therefore, valueless and proves nothing. I shall only add that, in this same chapter, he concedes the point in dispute when he allows, that 'undoubtedly a prolonged first stage is a bad preparation for any accidental complication of the second.' Why is it 'a bad preparation,' but because the patient is fatigued and worn out with unprofitable suffering, and the energy of the uterus is impaired?"—Pp. 127-136.

7. In the fifty-fourth, and afterwards more fully in the fifty-seventh volumes of this Journal, the nature and merits of the Principles and Practice of Obstetric Medicine and Surgery were placed before the profession. The opinions at that time expressed have been fully realized. The work has been most favourably received both in this country and in the United States. A second edition was published in 1846, as already noticed, in volume sixty-seventh;

and it is stated, that since the first appearance of the work, in its completed form, in 1841, the number of four thousand five hundred copies have passed through the hands of the publisher.

Though the extensive demand for a work on any department of medical knowledge, cannot in all instances be received as a proof and test of the merits of the work; yet, as in this instance, the merits of the work are undeniable, it is gratifying to know that they are so generally and unambiguously admitted.

In the present edition, Dr Ramsbotham has, with his great and intimate knowledge of the subject, exerted his usual skill and judgment in adapting the work to the advancing progress of midwifery as a science and as a liberal art.

In the second edition, published in 1846, Dr Ramsbotham introduced much practical information upon the *diseases of the puerperal and pregnant states*, and various points were illustrated by statistical tables from the practice of the Royal Maternity Charity. In the present edition, the author has added to this division of the work, and rendered it still more serviceable to the practitioner. This division extends from the 515th page to the end of the 667th.

In the present edition, Dr Ramsbotham introduces a judicious and temperate examination of the question of the induction of anæsthesia in labour, and the circumstances which may seem to indicate or to contraindicate the administration of chloroform. Dr Ramsbotham, after careful and dispassionate examination of all the circumstances, and considering the probable effects, arrives at the conclusion that chloroform is an agent which ought not to be administered in ordinary circumstances of labour to parturient females. His final conclusions are stated in the following manner.

> "No doubt it is a great bait to offer, and a great boon to promise to a woman who is looking forward with anxiety to her approaching confinement,—that she is to be lulled into a sweet, quiet, placid sleep,—to be perfectly unconscious of all the distress and suffering attendant on her labour;—and to be recalled to the world only by the grateful cries of her infant. But if the case were put fairly and honestly before them;—if they were informed that they might probably be made '*dead-drunk*,' but must certainly be reduced to that condition which the law designates as '*drunk and incapable*,' how many, it may be asked, of our high-born dames,—how many a woman possessing common feeling,—how many indeed removed above the very lowest orders of society, would be found to avail themselves of the immunity from suffering which anæsthetics hold out, at such a price, and at such a sacrifice of moral obligations? I cannot help thinking, and I may add also, hoping, for the honour of female nature, that those who have acceded to the proposal have been betrayed into acquiescence by the belief that they were to be merely steeped in sleep, and not drowned in intoxication. And yet nothing seems

more clear than that they must be cast into the intense stupefaction of drunkenness, before they can be rendered oblivious to the mental anxieties or corporeal sufferings incidental to their situation.

"I must repeat emphatically, that this state is not sleep, but drunkenness. The only kind of sleep, if sleep it can be called, in the least degree analogous to it, is that death-like insensibility into which a person is hurled when stupefied by spirituous liquors:—a state but little removed from apoplexy; and which indeed in very many instances has eventuated in a seizure of that dreadful malady. Even from the stupor induced by opium, a patient can be roused by loud and sudden noise,—as the firing of a pistol,—or by violent usage; but no power on earth could recal to his senses and sensibilities one overwhelmed by the deadening influence of the vapour of chloroform, so long as it retains within its grasp the unconscious object of its irresistible might."—Pp. 179, 180.

The whole discourse, however, should be perused, and will, we cannot doubt, be perused carefully.

On several other subjects, as the employment of galvanism in lingering labours, the operation of turning in cases of head presentations where the pelvis is small, and on the removal of the placenta before the infant, where that body is implanted over the *os uteri*, Dr Ramsbotham has added various instructive observations.

In conclusion, this work sustains with great ability the character originally given. It may be justly recommended as a Treatise on Obstetric Medicine and Surgery of a very superior order.

ART. III.—1. *Translation of the Pharmacopœia of the Royal College of Physicians of London,* 1851. *With Notes and Illustrations.* By RICHARD PHILLIPS, F.R.S., L.E., F.G.S., Late President of the Chemical Society, Curator of the Museum of Practical Geology, &c. London, 1851. 8vo., Pp. 567.

2. *The Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias. Comprising Standard and Approved Formulæ for the Preparations and Compounds employed in Medical Practice.* By HENRY BEASLEY. Fifth Edition. Corrected, Enlarged, and Adapted to the Last Editions of the Pharmacopœias. London, 1851. 12mo., Pp. 546.

IN the article in last volume on the London Pharmacopœia and the Commentaries on that publication, no information had been communicated to us, whether the late Mr Phillips had been engaged in a translation of the Pharmacopœia or not. It was consequently not in our power to speak with any confidence at that time as to the fact whether a translation was to appear or not. At the same time we expressed the opinion, founded on the previous services of Mr Phillips, that, had life been prolonged to this active chemist, we have no doubt that any translation coming from him would be received with great satisfaction by all members of the profession, and with that confidence which his reputation justly inspired.

Since the time referred to, the translation by the late Mr Phillips has appeared; and though it has been late, yet we lose no time in making the fact known, so far as lies in our power, to the members of the profession. The work was received only on Monday the eighth of December, eighteen hundred and fifty-one.*

It is stated in the advertisement, that the late Mr Phillips, at the request of a Committee of the Royal College of Physicians appointed to revise the Pharmacopœia, either conducted or inspected the preparation of most of the medicines which have been introduced, or the methods of preparing which have been altered, in the recently published edition of the Pharmacopœia. It is right that this should be generally known; since it cannot fail to make the Formulæ in the Pharmacopœia and the various directions given, to be received with much greater confidence than if they had not been subjected to this ordeal. Not only, therefore, has the new Pharmacopœia had the advantage of the services of Mr Phillips, but the present is, indeed, the authorized translation of the Pharmacopœia.

The present translation of the Pharmacopœia was undertaken, it appears, by Mr Phillips at a time when he might reasonably have looked forward to the completion of the duty by his own personal efforts. With his wonted activity and diligence, he had been enabled to write a large portion of the manuscript and the explanatory notes, when he was overtaken by the illness which terminated life.

The materials thus left were placed in the hands of the present editor, Mr J. Denman Smith, an able pupil of Mr Phillips, in order that he might superintend the work in its passage through the press. For performing this duty Mr Smith possessed peculiar qualifications, both as a well-informed chemist and one who had been long familiar with the opinions of the deceased on the leading chemical doctrines.

The present translation is constructed very much after the method of that of 1836; but it is both upon a more extended plan, and we think that it is decidedly improved. Both Mr Phillips and Mr Denman Smith have been anxious to present those new views of chemical action which the rapid progress of chemistry, cultivated as it is in different countries by numerous ingenious men, is incessantly bringing forward. In this duty we can say that

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* The Advertisement is dated September 1851 : the day is not specified.

Mr Smith has been both industrious and successful, and many of the commentaries given by him show how much the work owes to his knowledge. Of this, readers who peruse the volume with care, will observe frequent proofs.

On the important subject of Tests, a department in which the edition of 1836 was very complete, every thing has been done to render them both trustworthy, and at the same time of easy application. This subject acquires additional importance in the present work, from the circumstance, that as the College do not insist, that the medicines and chemical preparations ordered should be prepared in the exact mode prescribed in the Pharmacopœia, providing they are of the required purity and strength, the only mode of determining these qualities is by the use of accurate tests.

In the article in last volume, we stated in general terms, that several formulæ or directions for the preparation of chemical articles had been removed from the Formulary, either because they were supposed to be there out of place and unnecessary, or because the articles had been transfered to the Materia Medica List, and consequently were understood to be no longer prepared by the apothecary. On the propriety of this proceeding different opinions will be entertained by different individuals. It may be doubted, so far as the actual preparation of chemical medicines is concerned, whether any of them can be advantageously prepared, as in former times, by persons engaged as medical practitioners. But whether this be admitted or not, it is impossible to deny, that physicians and apothecaries ought to know how these articles are prepared, to understand the chemical principles by which their preparation is regulated, and should possess a sufficient amount of chemical knowledge to be able to say when they are pure or impure, well prepared or imperfectly prepared. A competent knowledge of chemical pharmacy is, in short, not only a necessary branch of medical education; but the possession of such knowledge invariably makes its possessor a more useful member of the profession than one without it.

With the importance and the truth of these facts the late Mr Phillips appears to have been deeply and justly impressed. The removal of all the formulæ of 1836 from the Pharmacopœia, would, we think, have taken away the material elements of a considerable proportion of useful, chemical, and pharmacological knowledge. To obviate this result, the method was adopted by Mr Phillips and adhered to by Mr Smith, of inserting in the present translation, not only all the Formulæ of the present new Pharmacopœia of 1851, but all the most important Formulæ of the edition of 1836. The reader will observe in various parts of the volume, *Formulæ* printed in Italian characters. These *For-*

mulæ are those of the Pharmacopœia of 1836, which Mr Phillips and Mr Smith, in accordance with the plan now mentioned, have retained. The theoretical principles of these Formulæ are explained in commentaries in the same manner as those of the new Pharmacopœia. It is proper at the same time to inform the reader, that in communicating this species of information, Mr Phillips and Mr Smith have not in all instances retained the old Formulæ of the Pharmacopœia of 1836. Keeping the object of instruction in view, they have, where the progress of chemical knowledge had furnished improved methods, introduced new *Formulæ*, distinguishing them, however, from those of the Pharmacopœia in the manner mentioned.

In another respect Mr Phillips and his editor have consulted the interest of chemical and pharmacological readers. With the view of rendering the present Work a Compendium of whatever is known regarding the chemical properties of every substance, and preparation employed in the treatment of diseases, and inserted in the Pharmacopœias, they have introduced under proper heads the physical and chemical history of various substances for which either no formulæ are required or none are given. For example, the description of the Cinchona Barks and of the Alkaloids derived from them are given under the head of Decoctions of Cinchona; those of Tartaric acid and of its potass salts, under that of Tartarized Antimony; the account of Tannic and Gallic acids under the head Tincture of Galls. Upon a similar principle an account of the chemical history of soap is given under the head of the *Pilula Rufi*, of which that article forms an ingredient. [P. 390.]

Regarding the Medical or Therapeutic directions, they are short, clear, explicit, and, on the whole, correct. A good Posological table is subjoined.

A table of articles which stood in the Pharmacopœia of 1836, bu which are omitted in the present Pharmacopœia, and of articles and preparations inserted in the present Pharmacopœia, gives a just view of the comparative difference between the two works. Though a list of this kind was given in our last volume, yet it may be serviceable to present the table from the translation.

A Catalogue of Substances occurring in Materia Medica, and also of the Preparations omitted in the present but inserted in the Pharmacopœia of 1836, *and of those inserted in the present but not occurring in that Pharmacopœia.*

OMISSIONS.	INSERTIONS.
Materia Medica.	*Materia Medica.*
Abietis Resina.	Acidum Gallicum.
Acetosella.	—— Tannicum.

Omissions.

Materia Medica.

Acorus.
Allium.
Althææ Folia.
Amygdala amara
Asarum.
Aspidium.
Aurantium.
Aurantii Flores.
—— Oleum.
Barytæ Carbonas.
Bergamii Oleum.
Brominium.
Calamina.
Calcis Hydras,
Cardamine.
Centaurium.
Conii Fructus.
Contrajerva.
Creta.
Dauci Fructus.
Digitalis Semina.
Euphorbium.
Ferri Percyanidum.
Hyoscyami Semina
Juniperi Cacumina.
Lactucarium.
Lavandula.
Lauri Folia.
Limones
Malva.
Marmor.
Marrubium.
Menyanthes.
Myristicæ Oleum *è nuclei destillatum.*
Olibanum.
Opopanax.
Origanum.
Plumbi Carbonas.
Porrum
Quina.
Rosmarinus.
Rumex.
Sabadilla.

Insertions.

Materia Medica.

Aloe Barbadensis.
—— hepatica.
Anethi Oleum.
Atropia.
Copaibæ Oleum.
Cupri Sulphas venalis.
Ferri Sulphas venalis.
Fœniculi Oleum.
Granati Radix.
Morrhuæ Oleum.
Myristicæ Oleum, *e semine expressum.*
Panis.
Rutæ Oleum.
Sabinæ Oleum.
Silex contritus.
Sulphur præcipitatum.
Thus.
Viola.

Preparations.

Chloroformyl.
Liquor Ammoniæ Citratis.
Atrophiæ Sulphas.
Liquor Morphiæ Acetatis.
———— Hydrochloratis.
Cataplasma Carbonis.
—— Sodæ Chlorinatæ.
Decoctum Gallæ.
—— Granati Radix.
—— Hæmatoxyli.
—— Pereiræ
—— Taraxaci.
Emplastrum Cumini.
—— Ferri.
—— Potassii Iodidi.
Enema Assafœtidæ.
Extractum Aloes Barbadensis.
—— Nueis-vomicæ.
Infusum Cinchonæ.
———— spissatum.
———— Pallidæ spissatum.
Linimentum Calcis.

OMISSIONS

Materia Medica.

Simaruba.
Sodæ Acetas.
—— Carbonas impura.
Spigelia.
Stannum.
Succinum.
Terebinthino Canadensis.
Testæ.
Toxicodendron.
Tussilago.

Preparations.

Aconitina.
Morphia.
Carbo Animalis purificatus.
Testæ Præparatæ.
Aqua Fœniculi.
Decoctum Malvæ compositum.
—— Veratri.
Emplastrum Ceræ.
Extractum Digitalis.
Infusum Pereiræ.
—— Scoparii.
—— Simarubæ.
Argenti Cyanidum.
Liquor Calcii Chloridi.
Ferri Iodidum.
Hydrargyri Oxydum.
—— Binoxydum.
—— Biniodidum.
—— Bycyndidum.
—— Sulphuretum cum Sulphure.
Plumbi Chloridum.
—— Oxydum hydratum.
Liquor Potasse effervescens.
Potassæ Bisulphas.
Potassii Bromidum.
Liquor Sodæ effervescens.
Mistura Assafœtidæ.
—— Cascarillæ composita.

INSERTIONS.

Preparations.

Liquor Arsenici Chloridi.
Syrupus Ferri Iodidi.
Ferri Carbonas cum Saccharo.
—— Ammonio-citras.
Vinum Ferri.
Liquor Sodæ.
Zinci Chloridum.
Pilula Aloes cum Sapone.
Sulphuris Iodidum.
Syrupus Cocci.
—— Violæ.
Tinctura Aconiti.
—— Belladonnæ.
—— Cinchonæ Pallidæ.
—— Ergotæ Ætherea.
—— Limonum.
—— Lobeliæ.
———— Ætherea.
—— Quinæ composita.
Ammoniacum præparatum.
Assafœtida præparata.
Cassia ——.
Galbanum præparatum.
Pix Burgundica præparata.
Prunum præparatum.
Sagapenum ——.
Styrax præparata.
Tamarindus præparatus.
Thus præparatum.
Unguentum Belladonnæ.
—— Conii.
—— Hydrargyri Nitratis mitius.
—— Opii.
—— Potassii Iodidi.
—— Sulphuris Iodidi.
Acidum Hydrosulphuricum, *recens præparatum.*
Ammoniæ Oxalas, *Crystalli.*
Aurum.
Cuprum.
Ichthyolla.

OMISSIONS.

Preparations.

—— Moschi.
Oleum Origani.
—— Succini.
—— Terebinthinæ purificatum.
Pilulæ Hydrargyri Iodidi.
—— Sagapeni compositæ.
Alcohol.
Spiritus Ammoniæ.
—— Lavandulæ.
Tincturæ Camphoræ
—— Cardamomi.
—— Guaiaci.
Unguentum Hydrargyri mitii.
—— Hyrargyri Biniodidi.
—— Veratri.

INSERTIONS.

Preparations.

Liquor Chlorinii, *recens præparatus.*
——Indigo Sulphatis.
Platini Bichloridum.
Poiassii et Hydrargyri Iodocyanidum.
Stanni Protochloridum.

We have now mentioned as fully as the limits of this notice will permit, the principal characteristics of this translation of the Pharmacopœia of the London College. The commentaries, which are to the student the most instructive part of the work, are given with great clearness and accuracy. With the explanatory diagrams it cannot be difficult for any one of moderate attention, to understand the correct views of chemical action in pharmaceutical products. We therefore recommend the work in strong terms to all those who are engaged in the study of chemical pharmacy. To those who already possess the Pharmacopœia, it must be useful as a commentary and explanatory guide. To those who do not possess the Pharmacopœia, it may serve as a substitute for that volume.

Of the Pocket Formulary by Mr Beasley, we have merely to say that it is one of those portable works of which we had occasion to speak in last volume. It is arranged in alphabetical order, by which means facility of reference and consultation is favoured. The different articles contain many curious and serviceable formulæ.

At the end is given a list of antidotes and articles employed as counter-poisons, a short account of weights and measures, and a table showing the proportion of active ingredients, as antimony, mercury, opium, and hydrocyanic acid, in certain compound medicines.

As a portable dictionary of Materia Medica and Pharmacy, the work is calculated to be useful to the practitioner who has already studied the subject, and perhaps, as the author expects, as a counter companion to the dispensing chemist.

Art. IV.—*Lectures on the Principles and Practice of Surgery.* By Bransby B. Cooper, F.R.S., Senior Surgeon to Guy's Hospital, &c. London, 1851. 8vo. Pp. 964.

A didactic work on the Principles and Practice of Surgery, from an hospital surgeon of long and ample experience, and acknowledged eminence, cannot fail to be an acceptable present to students and practitioners in surgery. It is highly creditable also to Mr Cooper, that in imitation of the example of his uncle, he has held the office of surgeon to one of the first hospitals in the empire, not for his own benefit merely, but has shown great industry and activity in rendering his experience beneficial both to contemporaries and to juniors.

The latter, indeed, is one of the principal reasons with the author for publishing this volume; namely, that he might preserve for the use, both of former, present, and probably future disciples, some record of the useful lessons in the practice of surgery, which, as surgeon and teacher in Guy's hospital, it has been his happiness to convey. Judging from the past, we are inclined to say, that if the profession had at stated intervals, from the surgeons of each of the large metropolitan hospitals, only one such record, these would form most serviceable books to the reflecting surgeon. With how much interest are the few observations left by Sharp and Broomfield still perused; and coming to some recent times, it is superfluous to speak of the services of John Abernethy, Mr Laurence, Mr Travers, Sir Astley Cooper, Sir Benjamin Brodie, Sir Charles Bell, and Mr Vincent.

It is in accordance with the principles now referred to, that Mr Cooper wishes the reader to bear in mind, that it has not been his intention to write a systematic work on the Elements of the Science of Surgery. His object is rather, after the manner of an hospital teacher, to present a view of the present and actual state of the practice of the art, not only in his own experience but in that of the most eligible acknowledged authorities.

The work of Mr Cooper consists of an introductory discourse and seventy-five Lectures.

The first five Lectures are on the elementary subjects of the Blood; the Blood-vessels, and the Absorbents; Constitutional Irritation; and Inflammation.

Then follow Hydrophobia in the sixth, with the details of three illustrative cases (see Medical Intelligence); Tetanus in the seventh; Gunshot Wounds in the eighth; Burns and Scalds in the ninth; Suppuration in the tenth; Ulceration in the eleventh and twelfth; Mortification in the thirteenth; and Rose in the fourteenth Lecture.

After the author has considered at due length the pathological history and the therapeutic principles of these general and common subjects, he enters on the history of the proper and specific Surgical diseases.

First come Diseases and Injuries of Bones. The ordinary Diseases are explained in the fifteenth and sixteenth lectures; the Malignant Diseases in the seventeenth. The six following lectures, from the eighteenth to the twenty-third inclusive, are devoted to the extensive and complicated subject of Fractures.

In the twenty-fourth, twenty-fifth, and twenty-sixth Lectures, is delivered the pathological history of Diseases of the Joints, with the treatment appropriate to each. In the next four Lectures, from the twenty-seventh to the thirtieth inclusive, Mr Cooper explains the peculiar characters of the various forms of Dislocation in the superior and lower extremities.

In the seven following Lectures, from the thirty-first to the thirty-seventh inclusive, the important subject of the Surgery of the Regions is considered. In this division Mr Cooper explains fully the diseases and injuries incident to particular regions of the human body, and describes the treatment, whether medical, surgical, or operative, which each affection and accident demands.

Hernia and its different varieties are fully delivered in four lectures, from the thirty-eighth to the forty-first inclusive.

Next follow Diseases of the Rectum, in lectures forty-second and forty-third; Diseases of the Genito-Urinary system in lectures forty-fourth and forty-fifth; and the extensive subject of Stone in the Bladder in the next four lectures to the end of lecture forty-ninth.

Four lectures are devoted to Diseases of the Genital Organs; three to Diseases of the Breast; seven to Lesions of the Vascular System; one to Neuralgia; one to Tumours; six to Amputation; one to Distortions of the Limbs; and three to the Venereal Disease.

It is impossible in this place to enter into any detailed account which could do justice to the contents of this volume. We feel it, however, to be our duty to say, that Mr Cooper has provided for the surgical reader a volume full of instruction. The author expresses himself in very diffident terms as to the success of his work; and though he thinks it has merits as a practical performance, he seems willing, if the profession do not confirm this opinion, that his work should go into the oblivion which it would in that case deserve. This apprehension, we trust, will not be realized. If the book is neglected, it must be owing to causes unconnected with its actual merits as a didactic work, which, in our opinion, are very great.

ART. V.—*Dr Hooper's Physician's Vade Mecum: or a Manual of the Principles and Practice of Physic.* New Edition. By W. GUY, M.B., Cantab., &c. Lond., 1851. 12mo. Pp. 576

IN the year 1846, Dr Hooper's work was published with various additions and improvements by Dr Guy; and in the beginning of 1847, we took occasion, in our sixty-seventh volume, to direct the attention of readers to the services of Dr Guy, in accommodating the manual of Dr Hooper to the state of medical education, and the requirements of medical students, twenty-three years after the original book had been last published by its author.[*] It is not difficult to imagine the arduousness of the duty undertaken by Dr Guy; and perhaps he might more easily have written a new treatise.

On the present edition we have not much to add to what was then said. Dr Guy has evidently done all that an intelligent and industrious editor can do, in order to render the work useful to the student. He has introduced into the present volume, from all the best sources, whatever new information may throw light upon the nature and treatment of diseases. It may be justly recommended as a treatise, containing within its limits a compendious and serviceable view of the present state of special pathology and therapeutics.

Death of DR BADELEY, *of Chelmsford, on Monday, 22d September* 1851.

THE Profession have no doubt learned with unfeigned regret the death of Dr Badeley of Chelmsford under circumstances very distressing. The occurrence took place on Monday, the 22d September, at the residence of the deceased in New Street, Chelmsford. The following are the circumstances stated:—

On Sunday afternoon Dr Badeley was suffering under a severe attack of toothach. As the pain continued intense and without intermission during the evening and night, about four o'clock on Monday morning, it is said, Dr B. went into the Surgery for the purpose of procuring some medicine that might alleviate his sufferings. In doing so he took a quantity of Morphia. When the fact was known and the effects of the narcotic were appearing, a telegraphic message was immediately despatched for Mr Bransby Cooper of London, and all the members of the Profession in Chelmsford were in the meantime summoned. All attempts to restore sensibility proved abortive. Life continued until one o'clock P.M. on Monday, when respiration ceased.

Dr. J. C. Badeley was a well-educated physician, trained at Cambridge; a man of cultivated mind, much learning, and enlarged views. He had been appointed to deliver the Lumleian Lectures at the College of Physicians of London, for eighteen hundred and fifty-one; and the subject which he selected was On the Reciprocal Agencies of Mind and Matter, and On Insanity. These Lectures, three in number, contain a large amount of interesting observation and remark on the constitution of the human mind, the development and forms of insanity, and the best methods of treating mental derangement. They show that Dr Badeley had observed and read much and profitably on the mind, sound and unsound.

It is humbling, as well as mournful, to think that a mind so intelligent and so highly cultivated, has been extinguished for the uses of this life, by circumstances apparently so trivial.

Dr Badeley was brother to the eminent Barrister of the same name. He leaves a family of ten children.

[*] Edinburgh Medical and Surgical Journal, Volume Sixty-seventh, p. 303. 1847.

PART III.

MEDICAL INTELLIGENCE.

I. MEDICAL PATHOLOGY.

On an Affection of the Skin, VITILIGOIDEA—*a. V. Plana, β. V. Tuberosa.* By THOMAS ADDISON, M.D., and WILLIAM GULL, M.D. (Guy's Hospital Reports, Vol. vii., Part ii., p. 265. 1851.)

Willan employed the name VITILIGO to designate certain white, shining, smooth tubercles, arising on the skin about the ears, neck, and face, terminating without suppuration. Bateman expresses the opinion that the disease is somewhat rare, and perhaps but little known. He, however, gave a figure, and subjoined a description; some of the characters of the last of which the present authors think approach to the appearances which they have observed. They remark the discordance of some of the French dermatologists; and they think that the keloide of Alibert and Rayer is altogether different.

Drs Addison and Gull describe the characters of five cases.

1. Several years ago, a young woman, aged twenty-four, was admitted into the hospital with a peculiar eruption, extending across the nose, and slightly affecting both cheeks. It consisted of shining tubercles, varying from the size of the smallest papule to that of ordinary acne. They were of a lightish colour, with here and there superficial capillary veins meandering over them, giving them a faint rose-tint. The changes they underwent were very slow; whilst some advanced, others subsided, the further course of the case was not ascertained.

A model of this eruption is preserved in the hospital museum.

2. In the winter of 1848, the following case occurred.

Mrs B., aged forty-two, of fair complexion and blue eyes, married, mother of eleven children, had been the subject of jaundice for two years, with much pain about the right hypochondria. After the jaundice had lasted fourteen months, a change began in the integument, about the eyelids, and in the palms of the hands and flexures of the fingers. The skin was at this time of a lemon tint. The affection of the eyelids consists of patches of a light opaque colour, with the surface and edges slightly raised, extending from the middle of the upper lid inwards around the inner canthus, and then outwards along the lower lid to nearly the same extent. There is a small isolated patch at the outer canthus. The disease affects both eyes equally and symmetrically, with the exception of two spots in the right lower lid, about the size of a hemp-seed, more elevated than the rest. The cuticle over the affected parts is healthy. There is no appreciable induration. The patches are more sensitive than the surrounding parts. The capillaries of the cheeks are slightly tortuous. The palms of the hands are of an olive-brown; along the ridges on either side of the flexures, both of the palms and fingers, there is the same opaque yellowish discoloration. The appearance is much as if the cuticle were thickened, and the disease confined to it; but, on a complete investigation, it is evident that here, as on the face, it is healthy, and that the morbid change is seated in the cutis, which is rather thickened, altered in colour, and has increased sensibility. The disease remained stationary until death, at the end of four years from the beginning of the jaundice. Towards the end, the colour of the general surface deepened to a mahogany-brown. No affection of the skin, similar to that described on the face and hands, appeared elsewhere.

A model of this is also preserved in the museum.

3. On the 18th of August 1848, a patient, labouring under diabetes, was admitted into the hospital. The person, John Sheriff, was twenty-seven years of age, of middle stature, by occupation a tailor, residing near Kingsbridge, in Devonshire. About six months before, he began to pass an unusual quantity of water, feeling at the same time weak and feverish, with a dry harsh skin. On admission, he presented the ordinary symptoms of diabetes; he voided four pints and a-half of urine daily, sp. grav. 1050. The treatment pursued was various, but without any obvious improvement. On the 25th January of the following year (1849), the quantity of urine was seven pints and a-half, sp. grav. 1042. At this time an eruption somewhat suddenly appeared on the arms, at first apparently of a lichenous character. In the course of ten days it had extended over the arms, legs, and trunk, both anteriorly and posteriorly, also over the face and into the hair; it consisted of scattered tubercles of various sizes, some being as large as a small pea, together with shining, colourless papules. They were most numerous on the outside and back of the fore-arm, and especially about the elbows and knees, where they were confluent. Along the inner side of the arms and thighs they were more sparingly present, and entirely absent from the flexures of the larger joints. Besides the compound character produced by the confluence of two or three tubercles, many of the single ones had also a compound character, or appeared to have such, as shown by the prominent whitish nodules upon them. Some looked as if they were beginning to suppurate, and many were not unlike the ordinary molluscum, but when incised with a lancet, they were found to consist of firm tissue, which, on pressure, gave out no fluid save blood. They were of a yellowish colour, mottled with a deepish rose-tint, and with small capillary veins here and there ramifying over them. They were accompanied with a moderate degree of irritation, hence the apices of many were rubbed and inflamed. The nature of the eruption gave rise at the time to much discussion. On its first appearance, some suspected it to have a secondary venereal affection; but there was nothing in the case, nor indeed in the character of the eruption, when carefully examined, to support this view. The only cutaneous affection with which we could associate it was that of a young woman, whose case we have given above, where the tubercles had occurred in the face only. The eruption continued almost stationary from the end of January to the beginning of March, when many of the tubercles began to subside, having no obvious change in the texture of the skin. At the end of March the patient left the hospital, and the further course of the case was not ascertained. The appearances presented by the eruption in this case are well shown in model 2733th.

4. Eliza Parachute, æt. 33, of middle stature, moderately well nourished; mother of six children; catamenia regular. Her present illness began in 1848; she attributes it to fright, and to a blow received in the left groin whilst attempting to separate two men who were fighting. Two

days after this she became jaundiced, and had from time to time severe paroxysmal pains about the hypochondria, lasting for a day or two; the liver being also enlarged and tender. Four months after the commencement of the jaundice, (August 4th, 1848,) she was admitted into the Hospital under the care of Dr Hughes. She remained in until the 26th of September, and left much in the same state she was in when admitted. There was at this time nothing complained of beyond the itching and irritation of the skin common in jaundice. The present affection began after the jaundice had continued fourteen months, when she again came under the care of Dr Hughes. It first appeared in the hands, spreading across the flexures of the joints of the fingers and palms. Soon afterwards a yellowish patch of discoloration began near the inner canthus of the eyelid, and then a precisely symmetrical one at the same part on the opposite eyelid. These patches are very slightly raised, and not obviously indurated; they have extended very slowly. In the early part of the year 1850, two models 2733^{4th} 2733^{5th} were made of the case. At this time the patches on the face existed as above described. Along the ridges bounding the flexures in the palm and about the joints of the fingers, there were yellowish, opaque, irregular, and somewhat raised lines. About the thumb, first joints of the fingers, and inner and interior parts of the wrists, there is a gradual transition to a tubercular prominence of the affected parts, and some distinct tubercles exist on the elbow and knee. The diseased parts are tender, so as to give her pain in using a knife to cut bread. The whole surface of the body is of a dull lemon tint. Various means were employed without avail, the disease showing a tendency to progress slowly, Through the kindness of Mr Startin, under whose care the patient now is, we have been able to observe it up to the present time. The jaundice still remains occasionally deepened by the exacerbation of the hepatic symptoms. The skin is of a dull lemon hue. During the last seven months the affection has become more tubercular, especially about the back of the joints of the fingers of the right hand. The patch of confluent turbercles on the elbow (Plate II., fig. 2) has much increased since the model was taken Both elbows are similarly affected. There are also turbercles on the right knee, on the superior surface of the great toe, and on both ears. On the hands the gradations from the plane to the tubercular variety are well marked, and the essential relations of the two forms demonstrable. This case has been of the greatest value in enabling us to connect together the cases which had previously occurred. The tubercles about the ears, elbows, joints of the fingers, &c., are of the same character they were in Sheriff's case. They are firm, rather irregular on the surface; have much the appearance, at first sight, of small compound follicles, but on closer inspection are proved to depend upon a change in the cutis. On the surface small venous capillaries may be here and there seen, producing a mottled appearance. In the hands we pass insensibly from the tubercles on the back of the joints to the state described in Mrs. B's case. namely, the slightly raised, opaque, yellowish lines about the flexures of the palms and fingers. The further identity of the disease in the two cases is shown by the presence of similar patches about the eyelids in both.

Of this eruption a plate is given.

5. Mrs. J., æt. 43, of spare frame, and below the middle stature, married; mother of two children, and in good health, until about eight years ago, when her catamenia ceased, probably from fright. After their cessation she was never well, had pains about the right side and through the shoulders; and for several years past, indeed nearly ever since the commencement of her ailment, has been jaundiced. She was constitu-

tionally of a dark complexion; this has now become a deep olive brown. During the last five years there has been a gradual change in the integument of the eyelids, giving her a strange expression, which the artist has most carefully depicted. (*vide* Plate I). This affection of the skin began in the upper lid of the left eye, and extended round by the inner canthus to the lower lid. A similar affection then commenced in the right eyelid, and the appearances now presented by the two are remarkably symmetrical. The surface of the affected parts is slightly raised, and the edge defined. The colour is a light opaque yellow, " coloration feuille morte," with a mottling of the faintest rose tint, with a small meandring vessel or two especially on the patches, which are recent and extending. On passing the finger over the surface, there is a slight, yet but very slight, feeling of resistance. The older spots are the most raised. The cuticle is unaffected, and by slight tension of the skin, will be seen to pass unchanged from the normal to the diseased parts. The discoloured patches often smart, and to use the patient's expression, " seem as if gathering;" they have also an increased sensibility.

It will be observed, that as the disease extended, it has run along the lids so as to avoid the Meibomian region, and that in the left eyelid are two sebaceous follicles, enlarged and filled with dark pigment cells; during the last two years a spot of black pigment has appeared on the mucous membrane of the lower lip. The whole course of the disease has been very slow, and its increase, by degrees, almost insensible. There is no affection of the skin of any other part of the body, beyond the change in its colour above indicated. The liver is enlarged, and there is much tenderness about the left hypochondria. The urine contains bile: and the conjunctivæ are of a decided jaundiced tinge.

The authors have recorded these cases rather than giving any general dissertation on the disease. The connection of this affection of the skin with hepatic derangement is obvious; and the exception which took place in diabetes is important, in so far as modern pathology points to the liver as the defective organ in this disease.

Three Examples of Rabies Canina or Madness from Canine Poison, treated in different modes. (*Lectures on Principles and Practice of Surgery.* By Bransby B. Cooper, F.R.S., Senior Surgeon to Guy's Hospital. London, 1851. Pp. 69–73.)

Hydrophobia Treated with Morphia.

Edward Lynch, an Irishman, aged 26, was admitted into billet ward, under Dr Babington, on Sept. 26th 1837.

About five weeks before admission, a dog, which he had been thrusting at with a pitchfork, sprang at him, and bit his upper lip and cheek. He immediately seized the animal and strangled it with a hay-band. The bitten part of the lip was speedily excised, but not that of the cheek. Nothing material followed till Saturday, the 23d of September, when, after drinking two or three quarts of beer, he was taken with a fit of vomiting, and experienced great distress at the sight of some fresh meat.

On Sunday he was very unwell, but had no spasm; and in the afternoon he fell into a deep sleep, from which he was with difficulty awakened. On Monday morning spasms came on, and he became worse.

On Tuesday, at 9 A. M., he was admitted into the hospital. On admission his condition was as follows;—He had no pain anywhere, but experienced great lassitude and a sense of constriction about his throat. He took a table-spoonful of milk, but could drink no water. His voice was feeble, and resembled a loud whisper. The countenance was anxious, and he was spasmodically affected at the approach of any one, at such a time he appeared choking. Heart's action normal; pulse soft, irregular, and 60; tongue slightly coated, but moist; skin natural; his breath was of an acid odour, and every two or three minutes he was seized with violent sighing. At a quarter before 10 A. M. he had a turpentine injection administered. At 1 P. M. Dr Babington saw him, and ordered Ferri Sesq. Ox. ʒj.; Syr. Aurant. q. s. ut fiat bolus, to be taken every quarter of an hour. The spine to be rubbed with Tr. Lyttæ, and belladonna plaster to be afterwards applied to the stimulated part.

At half-past 2 he sank into a sleep, which lasted three quarters of an hour.

At 5 P. M. he was much prostrated, and the spasms increased. Pulse 54, irregular, and intermittent. He experienced the feeling of a ball rising in his throat. His urine, which he passed three or four times, was rather high coloured.

At 7 P. M. he was more restless. Mouth parched; skin hot and dry; pulse 64. The boluses were divided into three parts, and after each he took about two ounces of milk, the only nutriment taken since his admission. He has great difficulty in swallowing, and is very much convulsed.

At 10 P. M. his pulse was 54, but rather fuller; he could bear no light in his room, nor any one to approach him except his wife. His voice was natural, and he had no rigidity of muscle. He was ordered Morph. Hydrochl. gr. iij. every half hour till some effect should be produced. Unguent. Lyttæ was rubbed in over his chest about the region of the diaphragm, and afterwards belladonna plaster. He soon became more quiet, and slept from eleven till three. He passed a quiet night upon the whole, crying out at intervals. At 5 A.M. Sept. 27, he felt rather sick. He asked for a cup of milk, but swallowed it with difficulty. The bowels had not been open since his admission. The morphia was repeated.

At 8 P. M. he asked for some water, swallowed about two ounces, but immediately jumped up in bed and appeared choking. The morphia was repeated.

At 10 P. M. he was very violent and delirious. Pulse 120. He wished for water, but could not swallow it. Dr Babington ordered the Morph. Hydrochl. to be increased to four grains every half hour. He was confined to his bed with sheets.

12 P. M.—He still cries for drink, which, when taken, makes him sick. Pulse 144. The voice is thick. Breathing more laborious, and feeling of depression much increased. He displays no greater horror of fluids than of solids. Dr Babington ordered the dose of Morph. Hydrochl. to be increased to eight grains every half hour, and a tobacco enema to be given immediately.

About two o'clock the countenance was livid, and the angles of the mouth constantly raised and lowered. He made a noise like the howl of a dog. He breathed only eight times in a minute. Pulse small, 180. Occipito-frontalis contracted. He appeared unconscious, but now and then a calm smile played upon his face, as though his ideas were of a pleasing nature. The pupils were much contracted, and inobedient to the stimulus of light. Conjunctiva injected. He twitched the bedclothes, and pulled his neck about. From this time he gradually became worse till twenty minutes past three, when he died. Immediately after death his pupils were much dilated, and the corpse had a very cadaverous odour. A vein was opened, but did not bleed. The next morning, however, he was deluged in blood. He took altogether forty-eight grains of hydrochlorate of morphia.

Hydrophobia treated with Lead.

Samuel Soring, a Londoner, aged 35, was admitted into billet ward, under Dr Addison, on the 1st October 1837. He had formerly been addicted to drinking, but not of late years. For the last three weeks he had been in a desponding mood, so much so, that he occasionally kept his bed for a day or two, though he continued his employment, which was fish selling. About three months since he had been bitten by a spaniel. The wound was trifling, did not bleed, and caused only slight pain. On the day before his admission he was very ill, and spent a restless night. His symptoms on admission were;—An anxious and dejected countenance; breathing difficult and convulsive, and this more violent after any movement, draught of air, or sight of water; pulse 80, with little power; skin natural; tongue parched; great thirst; slight pain in the head; pupils contracted; bowels not opened since yesterday morning; a sense of suffocation and tightness across the throat. At half-past 10 A. M. Dr Addison saw him, and ordered him to be cupped over the epigastrium. The cupping caused so much excitement, that when eight ounces had been drawn, it was thought advisable to desist. Ordered also, Haust. Sennæ c. Ext. Col. c. ʒj. as an injection, and two drops of croton-oil to be taken every half hour till it operated. The enema was soon returned with a small quantity of feculent matter. He swallowed his medicine with great difficulty, wishing to have his eyes closed with while taking it. A composition of Ext. Belladon. ʒij. and Ung. Hyd. ℥j. was rubbed on his throat. At 3 P. M. Dr Addison again saw him. The croton-oil had acted powerfully, the stools watery and green; skin cold and wet; pupils dilated; urine scanty; and priapism present; purging still violent. Ordered Vin. Opii ʒj. statim, and Liq. Plumb. Diacet. *m*xl. every hour. The purging ceased shortly after taking the opium. At 10 P. M. Dr Addison again saw him. After the first three doses of the lead he seemed more tranquil. He now complained of great thirst, and drank eagerly two half pints of porter. He still went on regularly with his medicine, which he was persuaded to take with great difficulty. He was constantly troubled with violent emotions, which became worse after each dose of the medicine.

At half-past 2 A. M. of the following day he was much worse. He appeared greatly distracted, imagining himself surrounded by hideous objects. It was necessary to confine him to his bed, as he would not stop there without compulsion. After this he became unconscious of everything around him. He roared in a most awful manner. The muscles of his face were horribly distorted. A large quantity of frothy saliva poured from his mouth, his lips became livid, and he appeared writhing in an excess of agony. In this state he remained till half-past four, when he became suddenly quiet, and appeared quite exhausted. Five minutes afterwards he was dead.

Hydrophobia treated with Prussic Acid.

Emmanuel Soult, aged 7, was admitted into King's College Hospital, under Dr Todd, on the 22d of November, 1841. Two months before admission he had been bitten by a mad dog under the right eyelid. The wound bled profusely, but rapidly cicatrized, and no constitutional disturbance was manifested till the day preceding his admission. He then exhibited a strangeness of manner, and slight symptoms of pyrexia only, until, a few hours after his illness had commenced, he suddenly threw himself into his mother's arms, and screamed loudly. His eyes were bright and prominent, he frothed at the mouth, and his inspiration was jerked and audible. He tossed his arms about, and tried to vomit: refused to eat, drink, or lie down, but did not object to the presence of water. At night, the symptoms were the same, but exaggerated: and on the following day, after his admission into the hospital, spasmodic twitching of the muscles of the face, neck, arms, and larynx was observed. He vomited saliva and viscid mucus. Over the chest was heard a loud muco-sonorous râle. The pulse was small, and 140. The skin was dry, but not hot. The tongue was maintained protruding from the mouth; there was also some intolerance of light. Pressure on the cicatrix of the wound did not occasion pain, nor was it red or swollen. The lower lip, at its right angle, was very tumefied, which his mother referred to his having bitten it. He evinced great sensibility to draughts of air, requesting those who spoke to him not to blow upon him.

At a quarter to 4 P. M., by Dr Todd's order, five minims of hydrocyanic acid (Ph. L.) were placed on his tongue. At four o'clock, the spasms being frequent, and the retching violent, the acid was repeated in the same quantity, and was continued every quarter of an hour till five o'clock. After the second dose he seemed better: he was more quiet; retching less frequent; and he passed water in bed copiously, and was sensible of its escape. This state lasted till four to five o'clock, when he again, on the entrance of several persons into his room, became excited. The spasms returned with increased violence, there being at one time slight opisthotonos. The retching was more constant, and he could no longer lie down. At five o'clock, Dr Guy, in Dr Todd's absence, directed 20 minims of the acid to be given; and five minutes after, 10 minims more, but without any obvious effect.

At half-past five, Dr Todd ordered another 20 minims of prussic acid. This producing little or no good, at Dr Guy's suggestion Dr Todd ordered a frigorific mixture to be applied to the spine and throat. This soon had the effect of alleviating all his symptoms. He became more quiet; the retching was less frequent; he did not foam so much at the mouth. The larynx and pharynx were free from spasm; but his feet getting cold, and his pulse falling, the ice-bags were removed, and hot bottles applied to his legs. He swallowed some ice, too, with greediness, after which his power of deglutition improved so much that he was enabled to take some wine and Liquor Opii Sedativus. At seven o'clock, as he was again becoming excited, the ice was re-applied, but again removed at eight, since the heart's action was then much depressed. From this time till eleven, he was remarkably free from spasm, and his deglutition good. His tendency to delirium, however, increased and at eleven o'clock Dr Guy tried the cold douche. Almost immediately afterwards, the pupils, which had been previously dilated, became extremely contracted; and in a few minutes the boy was dead.

The body was examined fifteen hours after death, the autopsy revealing congestion of the brain, spinal cord, and their membranes. The cerebral substance was somewhat softer than usual. The bronchial tubes and lungs were also congested. Stomach empty and contracted; pharynx injected; its follicles being rather large.

II.—MEDICAL SURGERY.

Observations on the Treatment of Anasarca or General Dropsy, by Puncturing the Legs. By JOHN HILTON, F. R. S. (Guy's Hospital Reports, Second Series, Volume Seventh, Part Second, p. 358.) 1851.

IT is well known to physicians and surgeons, that the practice of puncturing the lower extremities in anasarca, though often attended with temporary relief, carries with it in general so much danger that it is almost uniformly deferred until there is no hope of relief by any other method, and when it is prescribed to be performed it has proved too often merely another mode of the fatal termination of the disease.

One of the most ordinary unfavourable results which tend to bring about the fatal termination is the occurrence of gangrene in the punctures, thence spreading rapidly over the limb or limbs. This has generally been ascribed to the previous great distension of the skin and cellular tissue and the great feebleness of the capillary circulation. In general bad sores were formed, and they in no long time became gangrenous.

Another evil, perhaps, is to be ascribed in some degree to mismanagement in certain situations. After the punctures have been made and the serous fluid begins to escape, it speedily wets the bed clothes so perfectly, that unless every means are taken to obviate the effects of this, the patient, by lying in wet sheets, becomes so cold and uncomfortable, that the remedy is nearly as bad or if not worse than the disease. It may be said that this is an evil which might by care be prevented. This is speculatively true. But it is so difficult, either in hospital practice, or among the lower ranks out of hospitals, to do so, that it is for practical purposes next to impossible.

Mr Hilton has seen in late years dropsical patients gradually approaching death, in great distress from difficult breathing and oppression of heart, yet who were much relieved of their distress by deep punctures made in the cellular tissue, and their end rendered comparatively calm. Some in whom death might have been apprehended next day have had life prolonged in comfort during days and weeks, by drawing off, through the same means from the areolar tissue, the dropsical fluid. Others in whom the state of the circulation and respiration, the coldness of the surface, unconsciousness, feeble whispering voice, suspension of the renal secretion, and deglutition performed with great difficulty, all denoted the speedy approach of dissolution,—Mr Hilton has seen a few hours after opening the cellular tissue by deep punctures, to be so much relieved as to be rescued from immediate danger. The cardiac action has become stronger; respiration less oppressed and more easily performed; the voice returns; deglutition becomes more practicable; the urinary secretion re-appears, unconsciousness restored, and heat return to the surface; and this amendment obtained by puncturing the limbs has been of such duration as to enable patients to live for many weeks, to move about, and enjoy the improved condition of health.

The relief thus obtained was effected by drawing the fluid from the areolar or cellular tissue of the whole body, by making punctures in the legs through the skin into the subcutaneous cellular tissue in such manner as to effect a communication between this tissue and the outer surface of the skin.

The instrument, the form of which is of some moment, which Mr Hilton finds most suitable, is a long, narrow, sharp-pointed lancet, fixed upon a cylindrical steel shoulder. The instrument punctures, but cuts or incises little.

The punctures should be made along the outerside of the leg or thigh, or both, about two or three inches apart. Four on the leg and the same number on the thigh are sufficient, or perhaps more than are necessary. The smaller the number the better, providing the object be attained. Two punctures on the thigh, and two in the leg are often enough.

The instrument, after perforating the skin, should be carried very obliquely from without to within, across the limb, or in a direction from below upwards, through the cellular tissue, between the skin and fascia covering the muscles, so as nearly to reach but not to wound the fascia. The object of this direction is to intersect the interfibrillar meshes of the areolar tissue to the length of one inch or more. The instrument should then be withdrawn through the same tract, care being taken to disturb the parts as little as possible, by external pressure with the hand. Extensive subcutaneous incisions of the cellular tissue, by any lateral movement of the instrument, are to be avoided. These are not required for the purpose, and they may be hurtful by injuring a tissue of feeble vital powers, and already weakened by distension with fluid.

Mr Hilton prefers the outer to the inner surface of the limb, because the former is less traversed by lymphatics and veins than the latter, and because, when the patient is in bed, the apertures occupy a lower level than would be obtained on the inner surface of the limb.

Immediately after the punctures have been made in one or both limbs, each limb should be enveloped in a separate blanket, for the purpose of maintaining a proper temperature, and to prevent chafing of the skin by the one limb being rubbed on the other.

If any appearance of irritation and maceration of the skin by the fluid take place, it is good to make the fluid preserve an exact course away from the limb, by fixing upon the limb worsted threads, which, acting by capillary force, should be arranged in such manner that, by entering gutta percha or glass tubes, they may direct the fluid into these, from which it may be conveyed into receptacles outside the bed.

Mr Hilton enters into several other details regarding the after-management. But these may be easily understood by those who are in any degree accustomed to treat cases of anasarca in this manner.

The method proposed, and already tried by Mr Hilton, seems well calculated to obtain the advantages, and avoid the disadvantages of puncture, when it is deemed requisite to have recourse to this rather ambiguous remedy.

The great object, after the operation, should manifestly be to keep the patient as dry and as warm as possible. From some experience in cases of this kind we may be allowed to say, that this object will be most easily and certainly attained by placing below the patient a sheet, in two or four folds, and below this a large piece of oiled silk. This arrangement will enable the attendants to change the linen, if wetted, without causing general disturbance of the position, often a troublesome matter to dropsical sufferers. The centre of the bed also should be well elevated.

III. SURGICAL PATHOLOGY.

Case of Popliteal Aneurism cured by Pressure upon the Femoral Artery. By G. M. Humphrey, Esq., Surgeon to Addenbrooke's Hospital, Cambridge.. (From the Provincial Medical and Surgical Journal for October 1851.)

William Mitchell, aged 24, a seaman, from Wisbeach, was admitted into Addenbrooke's Hospital, on August 1st 1849, in ill health, with anxious pained expression, sallow complexion, pulse 104.

The ham and lower-third of the inside of the right thigh was occupied by a firm swelling, giving a circular measurement of eighteen inches and a-half; the left limb, in a corresponding situation, giving only thirteen inches and a-half. The swelling was most prominent a little above the internal condyle, and at the middle of the ham, uniformly firm, but communicating an indistinct sense of fluctuation. Its outline could not be clearly ascertained in consequence of an œdematous condition of the subcutaneous tissues. The integuments were natural, with the exception of a faint blush in places, and a few white starry spots, like cicatrices, which he said had appeared within the last few days. There was effusion into the knee-joint, raising the patella and still further distorting the limb. The part was exceedingly tender, and he suffered great pain in the swelling, and agonizing pains shooting down the limb; the skin of the leg and foot feeling, to use his expression, as if it were being burnt in a fire. He was restless, incessantly turning about to find a more easy position. The limb below the knee was œdematous, rather cold, and devoid of sensation below the middle of the leg; he could not move the foot or toes. The femoral artery could be traced to the lower third of the thigh, where the swelling began. No pulsation could be felt in the anterior or posterior tibial arteries. No pulsation whatever could be felt in any part of the swelling, nor was his severe pain in the slightest degree relieved, or the size of the swelling diminished by pressure upon the artery at the groin, though it was continued for several minutes, so as to prevent the passage of blood. A distinct bruit was audible over the tumour; and there was also a bruit, synchronous with the first sound of the heart, heard over the commencement of the aorta.

Seventeen weeks before his admission he had been compelled to leave his ship in consequence of an attack of rheumatic fever, which continued severe for about five weeks. During the whole of that time he suffered a good deal of pain about the right knee, more particularly in the neighbourhood of the inner condyle of the femur. When the pain subsided in other parts it continued unabated in the knee. About this time he first observed the swelling, which began on the inside of the ham; it increased quickly, and was attended with distinct beating, which he could see and could feel with his fingers. He was very clear upon this point, and said that several surgeons had examined the swelling and had perceived the beating within the last few days. It seemed probable, therefore, that the cessation of the pulsation was dependent upon some inflammatory changes in the aneurism or the surrounding parts caused by the journey to Cambridge.

He suffered acute pain all night in spite of fomentations and opium, and on the 2d was still in great agony. The œdema had rather increased. No bruit or pulsation could be perceived in the swelling. Twelve leeches and fomentation applied, and a grain of morphia given at night.

3d.—Less pain; swelling rather less tense; the bruit had returned; still no pulsation.

4th.—A further diminution of pain. The swelling softer and fluctuation more evident. Pulsation could now be distinctly seen and felt; but it was limited to the course of the popliteal artery, not being perceptible in any other part of the swelling.

7th.—The pulsation of the whole tumour being to-day quite decided and the bruit audible, there remained no longer any doubt as to the nature of the case; so we determined at once to commence treatment by pressure. The arch tourniquet (Santorini's) was applied a little above the middle of the thigh, and the common tourniquet at the groin; the latter being retained in place by a strap round the pelvis and another round the upper part of the thigh. Great pains were taken to stop completely the pulsation in the artery, and in the tumour, and it required considerable pressure to do this. However, the tourniquets being used alternately, the object was attained continuously for several hours without much inconvenience to the patient; and after fifteen hours of effective treatment, the pulsation in the tumour had entirely ceased. Constant pressure was kept up for two days longer; afterwards the tourniquets were applied only at intervals. In three or four days, when the swelling had begun to diminish, he was greatly troubled with pains in the feet and toes. This was probably dependent on some change going on in the nerve consequent on the partial remission of pressure upon it as the aneurism decreased in size, for on August 30th the pain had nearly ceased. Still there was no sensation below the middle of the leg, and no power to move the foot and toes.

September 4th.—No pulsation or bruit. The size of the tumour decreases slowly. The movements of the knee are much more free.

October 4th.—Has for sometime moved about the ward on crutches swinging the right leg. Moves the knee boldly though not quite freely, The size of the swelling diminishes very slowly, the circular measurement being still 17 inches. It is soft and indistinctly fluctuating. Sensation exists over a greater space than it did, but he cannot move the foot and toes, which remain swollen. The femoral artery may be traced to the lower-third of the thigh. There is no pulsation perceptible in the tumour or in the tibial arteries. The œdema of the foot might prevent its being felt in the latter. Returned to his home.

In the course of the following winter he was again in the hospital for several weeks, on account of the paralytic condition of the foot, which continued, although the aneurismal tumour was smaller and soft, and pulsation had entirely ceased. Electricity was used, with some benefit, and he again went home. Some months afterwards he was attacked with inflammation of the chest, and died. Mr Burnam, who attended him at Wisbeach, wrote me word that he had recovered the use of the limb, and that the swelling in the ham had almost disappeared before his death.

In this case the origin of the disease in the popliteal artery was clearly traceable to rheumatism. It is very seldom that such a cause can be assigned for the commencement of aneurisms in the extremities; the disease being usually attributed to an injury, a sprain, or to free movement of the part. Even in the thorax the diseases of the arterial coats that give rise to aneurism are more generally dependent upon age or intemperance than rheumatism.

The extent to which the characteristic symptoms of aneurism were, on the patient's admission, masked by a tense inflammatory condition of the surrounding tissues, deserves particular attention. At one time both pulsation and bruit were entirely absent, and pressure upon the femoral artery failed to produce any alteration in the swelling; indeed the swelling then resenbled an abscess more than an aneurism, and it is certainly no wonder that mistakes in diagnosis should, under similar circumstances, have, in more than one case, led to serious mistakes in treatment. As the inflammation and tension diminished, first the bruit and then the pulsation returned.

The cure by pressure upon the artery above the aneurism was effected in a shorter space of time than in any other case of which I have seen an account; all pulsation in the tumour having ceased fifteen hours after the treatmont was comenced, and it never returned.* A case is related in Dr Bellingham's " Observations on Aneurism," treated at the Royal Military Infirmary, Dublin, in which pulsation ceased after ten hours of compression, maintained by two instruments alternately relaxed, (the pressure upon the artery being sufficient to check completely the pulsation in the tumour,) and it did not return. In this case, however, it is to be observed, that pressure had previously been applied to the artery in the groin by a weight, and had, it was supposed, caused some deposition of fibrin in the aneurismal sac. Not long ago a case was related at the Medico-Chirurgical Society, by Dr Bellingham, in which the pulsation had ceased twenty-three hours after compression had been commenced. In the greater number of cases of which reports have been published, weeks, and even months, of patient endurance and constant attention have been required to bring about a successful issue. Indeed the great, perhaps the only, valid objection to the treatment of aneurisms by compression of the artery above the tumour, is founded on the tediousness of the process, and the pain to which the patient is subjected by the long-continued pressure of the tourniquet. In all other respects there can, I think, be no question that preference is due to the treatment by compression in comparison with that by ligature. Its general efficacy has now been proved in a sufficient number of cases. Of thirty-six cases treated in this way, in Dublin, we are informed that twenty-nine have been cured.* If properly conducted, the procedure is almost entirely devoid of danger either to the limb or to the patient; and, if it fails to effect a cure, the patient has not been placed in a more unfavourable position for the treatment by ligature. In cases where it has been necessary to operate under such circumstances, or where an opportunity has been afforded of dissecting the parts after compression, it has been found with one or two exceptions, that neither the artery nor its investing cellular tissue have been subjected to any material alteration at the part pressed upon, so as to increase the difficulty of the operation, or to modify its results.

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* It has been already shown in this Journal that the shortest time for the cure by compression took place in the case by Dr Hutton after seven hours; and in one case by Dr Humphrey, after ten hours.—Edinburgh Medical and Surgical Journal, Vol. lxxvi., p. 232. July 1851.

From the history of cases, and the dissections that have been made, it appears almost certain that the cure of aneurism, whether the means used be compression or ligature, takes place in much the same manner, viz., by the formation of coagulum in the cavity, and the subsequent absorption of that coagulum and contraction of the sac. The formation of the coagulum appears, in most instances, to be the primary and essential part of the process, and the treatment is likely to be effectual and speedy, in proportion to the degree in which it favours the coagulation of the blood in the sac, by placing it at rest, It is by no means necessary that the artery should be obliterated at any part of its course. True, it commonly is obliterated in the vicinity of the aneurism; but in all the instances of dissection after cure by compression that I have met with, the artery has remained pervious above the aneurism; and even where ligature has been employed, it has not unfrequently remained pervious between the ligature and the aneurism. It is not even necessary that the circulation through the artery should be completely arrested, a mere retardation of the current of blood being sufficient to permit the deposition of fibrin in the sac, which is already favoured by inequalities in the surface of the sac and other causes.

Nevertheless it must be borne in mind, that the more completely the current is suspended, the more quickly will coagulation take place in the sac, and the more speedy, as well as more certain, will be the cure. Mr Humphrey think that this point has not been sufficiently attended to in the treatment of aneurism by compression. So much has been said about its being requisite only to moderate the force of the arterial stream, that surgeons have been content in many cases with applying very slight pressure, have thus prolonged the treatment over an unnecessary period, and have sometimes failed to produce the desired result altogether. This at least has, he argues, been the case in most instances where so much time has been required to effect a cure; or, what amounts to the same thing, the instruments have been clumsy and insufficient, or the patient intolerant of pain.† Surely more numerous and more speedy cures would be obtained, and the new treatment would lose some of the reproach which now attaches to it, if the attempt were made in each case to carry it out more vigorously and effectively. In doing this, great care and great attention is required. Two, or even three, tourniquets should be used, so as to distribute the pressure over a greater surface, and to permit of a variation in the points at which it is applied. The instruments should be well adapted, and the surgeon should visit the patient frequently to adjust the apparatus, and regulate the pressure. If it can be done, without giving much pain, the pressure upon the artery should be maintained with such force as completely to stop the pulsation in the tumour; and great pains should be taken to effect this. If that cannot be done, we should endeavour to effect this, as nearly as possible, by maintaining as much pressure as the patient is able to bear. In a great number of the recorded cases no mention is made of the amount of pressure employed; but so far as I have been able to gather, the cures have been far the most expeditious, and on the whole the least painful, where the surgeon has made it his object not merely to *moderate*, but to *stop* the pulsation of the aneurism.

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* At the time at which Mr Tufnell's work was published thirty-nine cases had been treated in Dublin by compression; and in thirty of these the cure was complete and permanent. Since that time one case was treated successfully by Dr Bellingham, and one by Dr Hargrave; making in all forty-one cases in Dublin, thirty-two being successful.—Edinburgh Medical and Surgical Journal, Vol. lxxvi., p. 500 and p. 505.

† Doubtless, as Mr Paget suggests in his remarks upon a case lately published in the Medical Gazette, the particular form of aneurism, whether it be "saccular" or "tubular," must have great influence on the facility with which coagulation of its contents will be brought about; the uniformity of the dilatation in tubular aneurism being very unfavourable to such a process. But this is not sufficient to explain the lengthened periods consumed in the treatment of popliteal aneurism by compression in so many cases, because the "tubular" form of aneurism is very rare in the ham.

IV. OBSTETRICAL SURGERY.

Case of Pelvic Tumour obstructing Labour, in which the Cæsarean Section was proposed; with Observations. By ROBERT SHEKLETON, M.D., F.R.C.S., one of the Presidents of the Obstetrical Society, and Master of the Lying-in Hospital. (Read before the Obstetrical Society, Session 1849-50.) From the Dublin Quarterly Journal of Medical Science. New Series, No. XX. November, 1850.

IN this paper Dr Shekleton details the circumstances of a case in which he conceives the Cæsarean Section might have saved the life of the patient.

The patient was a person named Ann Parsons, admitted at 4 A.M., on the 20th July, to the Dublin Lying-in Hospital.

The gentleman on duty reported that he could not discover any presentation, owing to a compact, firm substance filling up the cavity of the pelvis; and on examination this statement was confirmed, and the woman was then interrogated as to her age, number of children, progress of former labours, &c. We elicited from her, rather reluctantly, that she had been delivered five times in this hospital; and it was ascertained from the records of the house that her first child was dead born, her second was delivered by the crotchet, the third was acephalous, the fourth was extracted by the crotchet in December, 1841, and her fifth by the same means in January, 1846, three years and a half ago. The observation appended to her name in the year 1841 was:—" There is a large tumour of bony consistence growing from the back part of the pelvis, filling up the cavity all to a small space, through which the child was, with much difficulty, extracted." In 1846 the pelvic tumour was described as being of such magnitude and density as to convey the same sensation to the finger of the examiners as the fœtal head, after it had cleared the os uteri; and Dr Johnson (then Master) was of opinion that it had enlarged considerably since her last confinement in 1841. The details of the delivery of 1846 are, that the head was perforated, after a labour of fourteen hours, with no great difficulty; but it required two hours of active, unremitting exertion of three gentlemen to drag it through the pelvis; nor was this effected till after the entire calvarium had been removed, so that, when the extraction was finally accomplished, nothing remained of the head but the face and base of the skull. Notwithstanding the prolonged pain and suffering she underwent on that occasion, the pulse never flagged during the operation, and her convalescence proceeded so favourably, that she was able to return home on the ninth day.

This history comprises all the particulars of the last and former confinements of the patient, and shows the dangers of her present position. She was about thirty five years of age, strong and healthy. She stated that she had been in labour since 11 P.M. the previous evening, and that there had been some dribbling of the waters before her admission. On examination at 9 A.M., a large unyielding tumour was found to occupy the whole cavity of the pelvis, with the exception of a space immediately behind the pubis, which barely admitted the passage of one finger between it and the tumour, while to the right this space was somewhat larger, owing to the position of the tumour being more to the left side. Neither the os uteri nor the presentation could be ascertained. On examination of the abdomen the uterus appeared to lie obliquely, with its fundus inclined to the left hypochondrium, and its cervix pushed into the right iliac fossa, where the head of the child could be felt, hard, round, and slightly moveable, resembling very much a tumour, for which, indeed, it was for a time mistaken. The fœtal heart was distinctly audible about one inch below the umbilicus, in a line towards the centre of Poupart's ligament, and the placental soufflet was detected near the fundus.

On carefully considering all the circumstances of the past history and present state of this case, namely, the extreme straightness of the passage through which the child had to pass, even in the most mutilated condition as proved by the immense amount of difficulty that was experienced in her last confinement, and which might naturally be expected to be augmented by an increase of growth in the morbid structure since that time,—the strength and vigour of the child, as evinced by the stethoscope, and the certainty of its being the fifth sacrificed to the mother's chance of recovery,—the woman's naturally healthy constitution, guaranteed not only by her present state, but, to a certain extent, by her wonderfully rapid recovery on the last occasion,—the short time she was in labour, and the uterine pains being as yet neither very vigorous nor distressing, with a tranquil state of mind, great fortitude, and a pulse but 74; all these circumstances, affording presumptive evidence that this was a case in which the Cæsarean section might be performed most legitimately, and with the greatest prospect of success,—Dr Shekelton summoned a consultation for 12 o'clock, of the ex-masters of the hospital, Doctors Collins, Evory Kennedy, and Johnson, and Sir Philip Crampton, its surgeon, and in the meantime prepared a small ward well adapted for the operation, and had everything requisite for its performance ready before the hour appointed.

Dr Collins after a minute examination thought he could feel the *os uteri*. Dr Kennedy, after a lengthened manipulation, discovered the *os uteri*, which by this time had become dilated, as well as he could judge, to the extent of half-a-crown, with the head resting on the crest of the pubis. Dr Johnson thought the tumour had not increased in size since the woman was last under his care. Sir Philip Crampton examined, but expressed his unwillingness to give an opinion in a case so purely obstetric. It was then arranged that, as Dr Collins could not attend till half-past 3, P.M., an adjournment to that hour should take place, the woman in the meantime to be kept quiet, and have an emollient enema.

At half past 3 the same gentlemen met, with the exception of Dr Johnson, accompanied by Mr Cusack, Professor Harrison, Mr Hamilton, Drs M'Clintock, Hardy, Denham, &c. The pains since morning had become strong and frequent, recurring every three or four minutes; the pulse had risen to 100, and was full. On examination now it was ascertained that a small segment of the head could with some difficulty be reached, just sufficient to enable the perforator to be used; and the majority of the accoucheurs considering the possibility of perforation, and the great success that had attended the former deliveries as regarded herself, were of opinion that the head should be lessened, and time given to try how far the uterine efforts, which were now vigorous, might mould the collapsed bones to the space through which they had to pass. Feeling the great responsibility that would attach to me individually, if he acted in accordance with my own views of the case, in opposition to the judgment of such sound and experienced practitioners, and failed in an operation which they deemed ineligible, Dr Shekleton determined on yielding to their decision, and undertook the duty of carrying it into effect. Accordingly, he introduced the perforator, and made, as far as the space would allow the handles to diverge, the ordinary crucial incision. Judging that the crotchet would enable him to use more freedom in breaking up the cerebral mass, he substituted it for the perforator, and in a short time evacuated a large portion of the contents of the cranium. Five o'clock having now arrived, the attendants adjourned till 7, when Drs M'Clintock and Denham, who assisted Dr Johnson at the last delivery, and knew all the particulars of the case, afforded their aid, in addition to Dr Hardy and Drs Sibthorpe and Johnston.

On examination now Dr Shekleton found that the left arm of the child had fallen into the narrow space in the vagina, and no effort could return it, nor could a finger be passed up at the side of it to ascertain the position of the head; it was, therefore, evident that the case had become seriously complicated by this untoward change of presentation, and that no alternative was left but to separate the arm from the body, eviscerate the child, and deliver as best we could with the crotchet. Accordingly the instrument was fixed in the axilla, and such traction employed, that at length the arm gave way, bringing with it the scapula of that side. By degrees the whole of the thoracic viscera, the ribs, and the contents of the abdomen, were torn away, and finally the spine was unintentionally divided in the middle, and many of the vertebræ were extracted separately. After various attempts and failures two crotchets were at length firmly fixed in the fœtal pelvis, and by the united alternate efforts of the operators, they at length succeeded in dragging the lower extremities through the os externum, and with them the right arm, attached by a strip of integument and torn muscles. In this stage of the operation the funis got entangled in the crotchet, and the placenta came away with it in the efforts to extract the extremities, but no hemorrhage ensued. A most perplexing and difficult operation at all times, but especially so in the present instance, still remained to be performed, namely, the extraction of the child's head. In despair of finding any means by which to fix it at the brim, and grasp it by the crotchet Dr. Shekleton introduced his fingers into the vagina, and there found the stump of the dorsal vertebræ, with small portions of ribs attached, which he instantly secured, and firmly held till the crotchet was fixed at the back of the ear externally, and the head extracted by Dr. Denham with extreme difficulty, and in a very flattened form.

Thus ended the mechanical efforts by which delivery was at length effected, which occupied upwards of three hours at the last sitting.

Till within about twenty minutes of the final step in the operation, the pains appeared strong and energetic, aiding the efforts at delivery, but a great change was now manifest in the sufferer; vomiting set in; the pulse became weak and rapid; brandy and other stimulants were freely administered, without effect; the surface was bedewed with cold perspiration; she ceased to complain, except by subdued moans and sighs; in short, death was depicted in her countenance; and in ten minutes after the head was extracted, Anne Parsons breathed her last.

Autopsy.—Eleven hours after the scene thus detailed an examination of the body was made, and the causes of delay and death were clearly and satisfactorily presented. The uterus was lying at the right side of the abdomen, with its fundus reaching a little above the umbilicus, giving it the appearance of unusual bulk, but this was evidently owing to the entire organ being in the abdomen, instead of its lower third having sunk into the pelvic cavity, as in the ordinary state, immediately after parturition. The structure of the body was firm and thick, but towards the cervix and os it appeared flattened and much thinned in its structure, and was projected forwards by the fundus of the tumour which occupied the left iliac fossa. The Fallopian tubes and ovaries at either side were healthy. The bladder was small and closely contracted. The peritoneal covering of the anterior abdominal parietes was much injected; there were, however, no other signs of inflammation, if this could properly be called one. On lifting the fundus of the uterus forwards, a quantity of blood, with small portions of fœtal brain, was seen. This, no doubt, escaped from the perforation in the cranium in the first stage of the delivery, and ultimately made its way into the cavity of the peritoneum through an extensive laceration which was now discovered. This laceration occupied the posterior wall of the uterus, was about five inches long, running obliquely from the right downwards through the cervix uteri and upper part of the vagina; involving the vagina, however, in the largest portion of its extent. Blood was extravasated between the peritoneum and posterior abdominal walls. The rent in the peritoneum covering the uterus was rather more extensive than that of the structure of the uterus itself, from which, to some extent, the peritoneal tunic was separated by coagula. Thus far as concerns the immediate cause of death.

The tumour was a large firm mass, nearly filling up the entire aperture of the true pelvis, except a very small space on the right side, which space was evidently much enlarged by the great pressure to which the tumour was so long subjected during the operation. It was of a pyriform shape, and extended from near the point of the coccyx to the superior brim of the false pelvis. It was found to be immoveably fixed in its situation, being firmly united to the periosteum (except at its fundus and superior part of its anterior wall) and to some of the last sacral nerves, one of which appeared to enter into, and to be lost in its substance. A portion of the anterior wall of the sacrum had been absorbed, and a part of the tumour had extended into the sacral canal, and lay anterior to the cauda equina. The rectum was pushed completely out of its natural position, and lay in the right side of the pelvis, free from pressure. On cutting into the tumour directly from above downwards, it was found to be very fibrous and solid throughout, except at its fundus, where there was a small cyst, lined by a fine vascular membrane, and containing about four drachms of a reddish serous-looking fluid. This cyst, as well as a smaller one, containing about one drachm, near its apex, was not discovered till after death; every part of the tumour within reach, feeling so firm and resisting to the touch, that it was thought by all to be of bony origin and structure.

The chief object of Dr Shekleton in bringing this case before the Society was to elicit an opinion as to the propriety of having recourse to the Cæsarean section, in cases similarly circumstanced with the one now related. It is to be regretted that the unsuccessful results that attended the operation for many years had so enslaved men's energies and judgment, that they dreaded to undertake the performance of it, for fear of adding to the melancholy list; and to the present day, many are so wedded to early impressions, that they still look upon it as sealing the death-warrant of an unfortunate patient, and the perforator and crotchet are preferred.

Of late years, however, cases of recovery have multiplied, and men are beginning to inquire into the causes of former mortality, and to be disposed to believe that, if the operation be undertaken early, on a patient in good health, and before symptoms of constitutional disturbance set in, if it be dexterously performed, and the after-treatment be conducted with judgment, a fairer prospect of success may be anticipated than the annals of former years have transmitted to us.

Dr Shekleton thinks that could the result been foreseen in this case, and also another case slightly different as to the cause of difficult parturition, the Cæsarean section might have been the means of preserving life. The other case is given in the following terms.

2. This was a case of perfect occlusion of the vagina, the result of a former delivery, occurring in a healthy young woman's second pregnancy, the first being eight years before. On making an examination at the time of labour, the finger was arrested, about one inch within the vulva, by a firm, pursed-up, cartilaginous-feeling septum, composed of strong fibrous bands, as if a ligature had been tightly tied round the vagina, and neither bougie nor probe could detect the smallest opening. The child was alive and vigorous. Labour went on for twelve hours before a small opening was discovered, through which a bougie was passed, and then the blades of a long dressing forceps. By diverging the handles of the forceps the opening was gradually, but painfully, enlarged, sufficiently to admit the fore-finger to the second joint, when the os uteri was found dilated and the head presenting. Three hours after this, the labour pains, though strong and frequent, made no impression on the opening, which remained hard, cartilaginous, and cylindrical, with the head resting above it. In two hours more, no advance being made, nor sign of further dilation, the edges of the opening were carefully and freely incised by a button bistoury till three fingers were admitted, when the head descended, and a scalp tumour gradually filled the space. There it remained, however, till the violence of the pains threatened rupture of the uterus, when the head was lessened and allowed time to collapse. After three hours more of fruitless patience, the pulse becoming quick and the belly tender, with fixed acute pain behind the pubis, the crotchet was introduced, and, with very great difficulty, the head extracted; but ruptura of the uterus was the consequence, and she died in thirty-six hours after delivery. A rent, one inch and a half long, corresponding to the fixed pain, was found at the junction of the os uteri and vagina.

After referring to the cases given by Dr Radford, and which are recorded in the seventy-sixth volume of this Journal, Dr Shekleton reasons in the following manner.

If we give credit to the cases of success reported by many of the older continental writers,—some of whom, Roonhuysen for instance, who records the operation as having been performed by a physician at Bruge; seven times on his own wife, six of which she must have recovered and Bartholin, who tells of a surgeon's wife at Paris, who had been five times the subject of it; and Roussett, who knew it to be six times performed on the same woman,—we must conclude, that the practitioners of Great Britain are mere babes in this branch of obstetric science compared to them. But, if true, we may in some measure account for their success, by the fact of their having had recourse to it, not as a mattar of necessity, but of election, and therefore they operated earlier, and under more favourable circumstances, no doubt often unnecessarily, as we could show.

Dr Churchill has collected from foreign authorities 371 cases of Cæsarean section, out of which 217 mothers recovered from the operation, and 154 died; but perhaps not all of these from the operation itself. Out of 189 of these cases, where the results to the children were given, 139 were saved, and only 50 lost.

Now, out of fifty-two cases reported by British and American practitioners (as recorded in the second edition of Dr Churchill's Practice of Midwifery), fourteen mothers only were saved, and thirty-eight died. This difference in mortality can only be accounted for by the delay in the operation, for most of the unsuccessful cases were days in labour:—one twelve days; others ten, eight, seven, three, &c.

He further gives from the work of Dr Meigs a case of difficult labour from deformed pelvis, occurring in a lady with her first child.

The deformity of the upper strait of the pelvis was so great (barely two inches in its antero-posterior diameter), that a living child could not possibly pass through it. A consultation was had, and it was agreed to wait some hours to see what effect uterine efforts might have upon its progress. At length it was decided that something should be done: the child was alive and vigorous. "The different methods of proceeding which have been proposed in similar cases were considered, namely, the division of the symphysis pubis, the Cæsarean section, and cephalotomy. The first was deemed inapplicable. The Cæsarean section was thought to be attended with so much risk to the mother as almost to be necessarily fatal, and some of the most distinguished medical men, including Dr Physic, were decidedly opposed to its performance.

Dr Meigs states that both he and Dr. Lukens were of opinion that the operation of cephalotomy, if not altogether incompetent to the delivery, would be attended with as much risk to the life of the mother as the Cæsarean section, for it appeared to them impossible that the cranium could be brought through the superior strait without the most violent exertions and great danger of lacerating the cervix uteri and vagina. Cephalotomy, however, was with great difficulty performed, and like Elizabeth Sherwood, the patient recovered, and was about her usual occupations in three weeks. In two years after, this same patient again fell under Dr Meigs' care at the full term, having resisted the advice both of himself and Dr Dewees to allow premature delivery to be induced at the proper time, and now refusing to submit to the Cæsarean section, and again craniotomy was performed. He does not say how long the operation lasted in this instance, but it was of many hours' duration in the first. It fell to Dr. Nancrede's lot to have the care of this lady in her third confinement. The breech presented, and delivery "per vias naturales" was deemed impossible; but she was safely delivered of a living child by means of the Cæsarean operation, performed by Professor Gibson. In a subsequent or fourth pregnancy, the same operation was a second time performed successfully by Dr. Gibson, in presence of Dr. Meigs and others, and both mother and child were again happily preserved. Dr Shekleton relates this case briefly merely to show, that in this instance the Cæsarean section was twice happily performed as a matter of *election*, not of necessity, for it does not appear that the impediment to her two first deliveries had increased, but her children had to be sacrificed; and the lady, like unfortunate Anne Parsons, would, in all probability, have ultimately fallen a victim to the repeated employment of the perforator and crotchet.

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PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*On Disease of the Pituitary Gland.* By Robert Scott Orr, M.D., formerly Resident Superintendant of the Glasgow Royal Infirmary.

In the last Number of this Journal I reported a very interesting case of epilepsy, in which, among numerous other morbid appearances observed after death, the pituitary gland presented distinct traces of diseased action. Since the occurrence of that case, now more than ten years ago, my attention has always been directed, in every succeeding *post mortem* examination, where an opportunity of examining the head was permitted, to the state and appearance of the pituitary body, and I have neglected no opportunity of observing it with care and attention.

In a few rare instances, I have found it presenting various marks of disease. It is probable, however, that it is but seldom subject to morbid changes; at the same time, it is to be feared, that it is often overlooked in post mortem examinations. The attention of the dissector is generally given entirely to the appearances found in the other contents of the cranium, and as the pituitary gland cannot easily be removed along with the brain, it is either forgotten altogether, or neglected to be removed afterwards for examination. That this is the case is highly probable, from the fact, that it is scarcely ever noticed in reports of dissections, and in searching for cases illustrative of its diseases and pathology, I have found but little written on the subject.

The function of the pituitary body being entirely unknown to us, and its pathology having been hitherto so little studied, it is with the view of calling attention to the subject that I venture to submit the following remarks and cases. The subject is confessedly an obscure one. I shall not therefore indulge in any theoretical speculations, but adhere to carefully recorded facts, trusting that though these are somewhat meagre and scanty, they may not prove uninteresting.

With the exception of those recorded in the treatise of the brothers Wenzel on Epilepsy,* I have been able to find only one detailed case of disease of the pituitary body. They give an account of twenty cases, where the dissections appear to have been conducted with great care and minuteness, and in all these more or less disease was discovered in this body. All the patients had been distinctly epileptic; but as I do not mean to enter into the question as to the connection between this disease and the morbid lesions found, I content myself for the present with remarking that the coincidence is curious, and shall allude to them merely as specimens of the morbid anatomy of the structure we are considering.

Rokitansky,† in his work on Pathological Anatomy, divides the diseases of the pituitary gland that he has observed into three heads. Under the first head he comprises anomalies of size,—under the second, diseases of texture, and under the third adventitious growths. I shall attempt to follow him in this arrangement.

1. *Anomalies in size.*—" Increase," he says, " in the volume of the pituitary gland, may occur from congestion, inflammatory swelling and abscess, or from the presence of an adventitious growth, but he has never observed any actual hypertrophy of the gland."

Atrophy he has frequently found. " It accompanies in old persons, the same affections of the brain. It prevails especially in the posterior lobe, which, besides diminishing in size, becomes loosened and pulpy, and discoloured to rusty brown or yeast yellow. At the same time the anterior lobe becomes pale, withered, and tough. Moreover, atrophy is sometimes produced, in cases of chronic dropsy of the ventricles, by the pressure which is exerted upon the gland by the serous effusions, through the medium of the third ventricle.

" The *infundibulum* in like manner usually shares in the atrophy, and becomes pale, withered, and thin."

1. A few weeks ago, I met with a case of hydrocephalus in a female child of seven years of age, in which, after death, the appearances last described above were extremely well marked,

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* Translated by M. Breton. Paris, 1811.

† Rokitansky's Pathol. Anat. Trans. for the Sydenham Society, by Chas. H. Moore. Vol. iii. page 432.

s the case was altogether an interesting one, I shall make ology for detailing it somewhat at length.

ie patient had from infancy been always a delicate child, and lained more or less of her head. Dentition was, however, ed with tolerable ease. She had been ill eight days before called in to see her. The prominent symptoms were pain head, and constant vomiting when food was attempted to ken. I was sent for in consequence of a restless night with vation of all her symptoms, the supervention of deafness, onsiderable stupidity of manner and deportment.

examination I found her free from fever. Her pulse was tongue pretty clean and moist; bowels reported slow. was exceedingly restless and talked incoherently, laughing spoken to with an imbecile expression of countenance. She continued deaf. Her head was cool, pupils were much dilat- nd the vomiting was reported to continue.

ie head was shaved and a blister applied behind the left ear the parietal bone. A grain and a-half of calomel was directed given every four hours, and three drops of the tincture of in water three times a-day.

the evening the pulse rose to 120. The skin felt more nd feverish than in the morning. The bowels had acted . She spent a restless night with much incoherent talking.

the 10th day of her illness her pulse was 104, soft and re- skin cool, pupils dilated, tongue clean; she laughed idioti- when spoken to, and could not answer questions.

ie blistered surface was directed to be dressed with equal of mercurial and iodine ointment.

6 o'clock in the evening I was sent for in consequence of the n supervention of coma, which occurred immediately after an of vomiting. I found her quite comatose and apparently death. Her pulse was 64, irregular; pupils much dilated. nema with half an ounce of turpentine was instantly admi- ed, and mustard sinapisms were applied to the calves of the after which she almost immediately began to revive, and when d could say she felt better; the pulse now rose to 84.

blister was applied over the right parietal bone and behind ır.

half-past 10 P.M., she again became comatose, but could difficulty be roused, and answered "yes and no" to ques- The pulse was 68, irregular, pupils contracted and insen- to the light of a candle. The turpentine enema was still ied. The head was cool, and as yet no convulsions had red.

ie calomel was directed to be continued, and the turpentine ia to be repeated in two hours.

On the 11th day she still continued drowsy, but could be roused, and protruded the tongue when desired. She vomited three times during the night, and the bowels acted once. Pupils much dilated. Pulse 72, very small and feeble.

Both blisters were directed to be dressed with the mercurial and iodine ointment, and another blister was applied to the vertex.

In the evening, though she still continued drowsy, she had occasional intervals of consciousness, when she revived, looked about her, and answered "yes and no" to questions, vomiting continued unabated, pupils very much dilated. Pulse 64, of better strength, but still languid.

She continued much in the same state during the 12th day.

On the 13th day drowsiness continued. Slight convulsions commenced in the morning. When I visited her in the forenoon I found the arms and hands tremulous from convulsive spasms; they could not be flexed without considerable force being used, in consequence of rigidity of the muscles.

A blister was again applied behind the left ear, and the calomel was repeated.

In the evening she was quite comatose, the muscles of the arms were still rigid. It was evident she was fast sinking, and at half-past nine o'clock she died.

The following appearances were observed on dissection thirty-eight hours after death.

Head.—On removing the skull-cap, very strong adhesions were observed between the *dura mater* and calvarium, extending throughout the anterior half of its inner surface. The surface of the brain was unusually dry, without the least appearance of effusion.

On making a section of the brain, about three or four ounces of clear serum escaped from the lateral ventricles. The anterior part of the *fornix* and *septum lucidum* was softened. The choroid plexus was blanched and bloodless. There was a recent effusion of soft flaky lymph over the *Pons Varolii* and *medulla oblongata.* A considerable quantity of fluid escaped from the spinal cavity.

From six to eight hard yellow tubercles were found imbedded in the substance of the *cerebellum,* one of which was as large as a barleycorn. A single tubercle was also found in the *Pons Varolii* (a rare occurrence, Rokitansky, page 428), and another in the left hemisphere of the *cerebrum.*

The pituitary gland was unusually small, and did not seem to fill as it ought its cavity in the *sella turcica.* The *infundibulum* was small and filiform, and appeared to be blanched and bloodless; in short, both it and the pituitary gland were evidently much atrophied. The posterior clinoid processes of the sphenoid bone were thicker than usual, and were bent much forward, overlapping the gland.

Chest.—Both lungs were studded from the *apices* to the inferior borders with miliary tubercles. The right lung adhered to the upper part of the parietes of the chest.

There was some fluid within the pericardium. The heart was normal.

Abdomen.—The mesentery was studded with tubercles, varying in size from a horse bean to a pinhead. The liver and other organs were healthy.

About a fortnight after witnessing this inspection, I had an opportunity of contrasting the appearances in the pituitary gland and clinoid processes in this case, with those found in another female child of four years old, who died of head disease, and in whom these structures were quite healthy. In this case, though the child was three years younger, the pituitary gland was at least a third larger than in the former case; it was healthy in its structure and filled its cavity properly; the *infundibulum* was of the usual tubular funnel-shape and size, and of its natural pink colour; there was no thickening of the clinoid processes, and they were not bent forward as in the former case. I mention these circumstances to show, that in judging of the morbid appearances described, I have not trusted to my own recollection as to what should be the natural appearance of the structures under consideration, but have tested them by actual comparison with a healthy specimen.

In the first, the sixth, and sixteenth cases detailed by the Wenzels, the pituitary gland seems to have been in a state of atrophy.

In the sixth case it resembled a vesicle; in short, it wanted a small part of the posterior border of the great lobe and the greatest part of the small lobe. The place where this loss of substance had taken place was filled with the peculiar yellow matter to be afterwards mentioned.

In the sixteenth case the posterior lobe was small, shortened, and thickened. When cut, the two portions exhibited two places of dazzling whiteness and cartilaginous appearance. On raising a small fragment with a scalpel, a thick milky fluid flowed out.

These observations agree with those of Rokitansky as to the posterior lobe being most frequently found atrophied.

In their eighteenth case the Wenzels found the posterior apophyses extremely bent, and the pommel of the *sella turcica* very thick. This bending forward of the clinoid processes was very remarkable in the case of the little girl which I have just related.

Morgagni found the gland depressed, small, and extremely soft. Greding notes it as small in four persons out of twelve.

2. *Diseases of Texture.*—Under this head Rokitansky places first Hyperæmia and Anæmia.

The fourth and eighth cases given by the Wenzels appear to have been examples of *hyperæmia.* In the former the gland was of monstrous size, greatly distended, the superior surface of a deep-red colour, and all the vessels were distended with blood. The posterior lobe, which he says in adults is always grey, was of a deep-blue colour. In the latter the anterior lobe externally was somewhat white; when cut horizontally the middle was of a deep-red colour verging to black. The posterior lobe was of a grey colour within and without.

Rokitansky says, "Congestion of the pituitary gland is generally combined with the like condition of the *pia mater;* but in young persons it is occasionally observed alone. It occupies the anterior vascular lobe, and the *infundibulum.* The cluster of vessels descending on either side of the latter to the gland is injected, the substance of the *infundibulum* is redder than natural, and the turgid gland is loose in its texture, dark-red, and full of blood. In some very rare instances the congestion leads to apoplectic extravasation. I have observed small streaks of it associated with congestion of the membranes."

"The gland is anæmic in cases of anæmia of the membranes."

The distinction between congestion and the first stage of inflammation in an obscure structure like that of the pituitary gland cannot, it is probable, be made with great minuteness. I pass on to the second division under diseases of texture, viz.

Inflammation.—It is here interesting to observe how exactly the description given by Rokitansky of the inflammatory appearances, he observed, agrees with those found in the cases recorded by the Wenzels. Thus both agree in finding the anterior lobe most frequently inflamed, both agree in their descriptions of the swelling, dark reddening, and looseness of the texture of the gland, and also in the lymphy exudations they observed.

In their ninth case the Wenzels found the pituitary gland remarkable for its clearness and extraordinary redness; the anterior lobe presented all the appearance of inflammation. The posterior lobe was white, whilst at the age (24 years) of the patient from whom it was taken, it is always grey. On the borders of the anterior lobe was a bright-red substance, and within a white substance, which filled almost all the capacity of the superior part.

In their tenth case there was found between the lobes of the gland a thick white matter greatly resembling congealed and hardened lymph.

The marks of inflammation in their eleventh case were very distinct. In this instance the *infundibulum* was thicker than natural. All the vessels of the *pia mater* surrounding the pituitary gland were distended with blood. Round the place where the posterior lobe joins the anterior was a mass of muddy lymph a little hardened; cut horizontally there was found a deep red substance in its anterior lobe, and there was much lymph still half fluid, but already muddy and viscid in the cellular substance at the point of union between the two lobes, and which ran out when they were separated.

Their twelfth case presented equally distinct marks of inflammatory action. Here the *infundibulum* was singularly red, and this redness was prolonged on the superior surface of the great lobe of the pituitary gland. The posterior lobe was hard and solid and could not be crushed with the fingers. In the superior half of the anterior lobe was a greyish white spot the size of a *centime* as hard as the posterior lobe. A clear colourless fluid ran from between the two lobes when separated.

In Duncan's case, already referred to, and which I related in the last Number of this Journal, the pituitary gland presented distinct traces of engorgement and inflammation. It was large, red, and firm, and when removing it, seemed to start from its cavity as if it had been confined within too narrow limits.

Rokitansky has seen suppuration and abscess result from inflammation in the gland, the purulent matter in some cases being confined in cysts varying in size from a hemp seed to that of a hazel nut.

In the Dublin Quarterly Journal for November 1848, (No. xii). Dr Heslop records an interesting case of hypertrophy and abscess of the pituitary body, of which the following abstract may communicate an idea.

J. H., twenty-six years of age, an engineer of active habits, and subject to exposure to bad weather, was attacked with most acute pain in the head on the 11th of February 1848, after dancing almost all the night previously. Though up to this period robust, and in constant employment with the duties of his profession, he had been very delicate in early youth, and inherited a very strong phthisical predisposition. Subsequently, it was remarked to Dr Heslop by a friend who had opportunities of seeing the patient daily, that for six or eight weeks before, he had exhibited an extraordinary tendency to sleep. If he sat down, he used to fall into a heavy sleep, almost amounting to stupor; but the sudden supervention of the acute pain referred to was the first symptom that attracted attention to his condition. After using some domestic remedies for a day or two, such as constantly bathing his forehead with cold diluted vinegar, without benefit, he sought medical advice. He was then treated for bilious headach, with some simple medicines, and in a few days the pain was very much relieved, and the slight concurrent feverish symptoms disappeared.

He began at this period to examine his business papers, and committed some dietetic errors. These imprudences were followed by a return of the pain with renewed force. He was at the same time excited, restless, and talked wildly; or, on the other hand, he would lie in a state of semi-stupor, silent, and absolutely indifferent to every thing around him. His pulse became quick, and he lost all appetite. Still his attendant thought the case one of no great importance,—a kind of stomach derangement. In health he had a jocose, wayward, and half mischievous way about him; and it must be confessed, his wild talking appeared very easily to be only an exaggeration of his ordinary manner. In the midst of all this,—four or five mornings after the renewed attack,—he stripped himself perfectly naked, jumped out of bed, and passed a stool on the floor while standing before his attendant and the physician.

Consultations were held, mercury given vigorously, leeches and blisters applied. Nothing gave a decided check to the progress of the disease, and he fell into a state of profound coma. The pulse became intermittent and very slow, some days even below 40 in the minute. It was now believed that he was in fever. Matters certainly came to a *crisis* on the evening of about the eleventh day after the supervention of distinct symptoms. His pulse became almost imperceptible,—extremities cold,—hands convulsively clenched,—jaws so firmly closed that, but for the cavity left by the loss of a tooth, nothing could have been poured down the throat. Wine was given freely, yet with caution; another blister was applied to the back of the neck; and from this day a gradual improvement took place.

This improvement was ascribed to the mercury, which had commenced to affect the mouth, and the other therapeutic means which had been employed.

From this period commences the knowledge of the case by Dr Heslop.

With careful support and stimulation he rapidly rallied, and, though occasionally retrograding and showing some of his former bad symptoms, twelve days afterwards he was so rational and had so good an appetite, that Dr Heslop confidently predicted an early and complete recovery. Yet two symptoms remained which caused Dr Heslop some misgivings. One of these was an almost total loss of memory; a few minutes after taking a glass of wine he would utterly forget having had it. No evidence could induce him to believe he was in Ireland and not in Sussex, where he had spent a few days two or three years before. The other ill-ominous symptom was loss of power over the sphincters. Now and then he would give signs that he wished to pass a stool, but the rule was, that it was voided unconsciously and without warning; and when he occasionally did ask for the urinal, it was almost certain that he would pass water before the attendant could give the utensil to him, so sudden and urgent was the impulse to evacuate the contents of the bladder.

He was nevertheless deemed sufficiently well a fortnight afterwards to undertake a journey of sixty miles to Dublin; upon arriving in which city he was put under the care of Dr Stokes. It would be tedious to go into anything like a detailed description of the many phases of a chronic and ever-varying disease. It may suffice to state, that he soon altogether lost control over the sphincters, that he progressively became emaciated, and that his memory appeared to have almost vanished; still, when tolerably well, he retained a considerable amount of intelligence, and frequently answered questions quite coherently, though so unwilling to speak that whole days used to pass without his saying more than "yes" or "no" occasionally. The idea irresistibly conveyed to the mind was, that, had the single faculty of memory been perfect, the intellect would have been sound. With the exception already given, he had paralysis of only one muscle—the orbicularis of the right eye-lid, which for several months before his death he did not close, sleeping or waking. The acute pains in the head never forsook him. Sometimes several days passed over without these attacks, but they were rarely more than two days apart; these always came on suddenly, when the face instantly became of a scarlet hue. While strong enough, he raised his hands to the head and gave a look, and uttered groans, too distinctly pourtraying the agony of the moment. The paroxysm rarely lasted more than ten or fifteen minutes, and was always succeeded by a profound stupor of some hours duration. At various periods he suffered also from severe cramps of the lower extremities. Towards the end of August it became obvious that the strength was nearly exhausted; but he was destined to give another illustration of the law that chronic disease kills by giving the predisposition to an acute one. An obscure attack of pleurisy came on, of which he died on the 4th of September.

Inspection thirty Hours after Death.—The body was extremely emaciated. Upon taking off the calvarium the membranes covering the hemispheres of the brain appeared quite healthy; the inspector detached the brain from the base of the skull; when he arrived at the second pair of nerves, he found them almost surrounded by a softish, dark grey tumour, about the size of a large walnut, occupying the position of the pituitary body on the *sella turcica*, but extending much beyond the space usually held by that body both posteriorly and laterally. It was very carefully dissected from the *dura mater* and surrounding structures, but not without slightly lacerating it and giving exit to a small quantity of thick, dark red, purulent looking fluid.

Having taken the brain out of the skull, and turned its base upwards, the exact relations of the tumour were examined. In this position it filled the entire interpeduncular space, extending backwards to the *Pons Varolii*, forwards and laterally protruding into the commencement of the fissure of Sylvius; the *crura cerebri* were distinctly pressed *outwards*, so that the space just referred to was greater than natural; it could be easily raised from its position after dissecting off a few delicate cellular bands, having no other connection with the brain than through the pituitary body, which seemed lost in the tumour; or rather this latter seemed a *cystiform enlargement* of that body containing the purulent-looking fluid already described. Underneath the tumour was observed a small quantity of thin fluid, of a slightly reddish tinge, upon drawing off which it was remarked, that it was but the surface of a more deeply situated fluid, contained in the lateral ventricles. Twelve or fourteen drachms were measured, but probably an equal quantity lay on the base of the skull, of which no note was taken; in fact, the tumour occupied not merely the floor, but almost the entire cavity of the third ventricle; the anatomical relations of the former were precisely identical with those of the latter.

The quadrigeminal bodies, the inspector thought, seemed flattened in the antero-posterior direction by the pressure of the tumour.

When we add an increased firmness on section of the *medulla oblongata*, and slight opacity, with thickening of the arachnoid, over the middle lobes of the base of the brain, we have mentioned all the morbid appearances in the encephalon. But we must record the fact, that the bones entering into the formation of the middle fossa of the base of the skull had scattered over various parts of them, curious little stalactitic processes or exostoses, varying from one to three or four lines in length; the elevations, too, corresponding to the sulci of the middle lobes of the brain, were extremely sharp and angular.

Dr Heslop declines inquiring whether these had any, or what share in the production of the remarkable disease under which this gentleman suffered.

No tubercle could be found in the lungs. There was a considerable amount of recent lymph over the base of the left lung and corresponding surface of the diaphragm.

The thirteenth, fifteenth, and sixteenth cases of the Wenzels exhibited the appearance of purulent matter effused in various places. In the first of these it was situated in the middle of the great lobe; in the second, in the anterior lobe; in the third, a cavity formed by cellular tissue in the posterior lobe contained a thick milky fluid.

Several other cases are detailed by the authorities quoted, showing the results of inflammatory action. But the above are sufficient to show the general appearances found in connection with this morbid state.

3. *Adventitious Growths.*—Rokitansky includes under this head tubercle, cancer, and the peculiar amber-coloured exudation described by the Wenzels as found in a large proportion of their cases of epilepsy.

1. Tubercle, he says, is a rare disease in the pituitary gland, occurring only in combination with tubercle in other organs, especially in the lungs and brain. "It assumes," he remarks, "both the form of gray crude granulations and that of a mass of yellow tubercle which softens and suppurates." I find no case resembling tubercle of the gland in any of the Wenzels' observations.

2. Cancerous diseases of the pituitary gland, Rokitansky observes, are the most numerous of all its morbid states; and in every instance that he observed, the growth was of the medullary kind.

"In one case it was a lobulated encephaloid, of a loose creamy structure, enclosed in a fibrous (neurilematous) cyst.

"In a second case it was a vascular, reddish-brown, and elastic, but firm medullary growth.

"In a third case, a similar growth was found degenerated into a brown chocolate-coloured fluid, and contained in a neurilematous sac which projected into the pharynx through an opening it had formed in the base of the skull, by destroying the body and part of the greater wing of the sphenoid bone.

"In several cases the growth had reached a considerable size, and the base of the skull was destroyed, and the brain displaced upward and laterally to a corresponding and very marked extent. The growth in this gland was in several cases the only example of the disease in the body, whilst in others, on the contrary, it was associated with cancerous formations in other organs."

The annexed very interesting and well marked case of cancerous disease of the pituitary gland I met with when resident in the Glasgow Royal Infirmary, under the following very accidental circumstances. Happening one day to go into the inspection room of that institution with one of the hospital clerks, we found two bodies laid out for inspection. One of these had died in a ward to which the gentleman who accompanied me was attached. He hesitated as to whether he would perform a *post mortem* examination or not, saying that the patient died of exhaustion, and he did not expect to find anything of interest. I urged him to perform the dissection, at the same time offering my assistance, and being an exceedingly intelligent and zealous person, he was easily persuaded. The sequel will show how we were rewarded for our trouble. Before, however, detailing the morbid appearances, it may be well to preface them by the following short history of the case taken from the hospital journal.

2. " ——— Teesdale, widow, aged forty-six, was admitted into the Glasgow Royal Infirmary on the 6th of February 1844.

" Four months ago had fever, since which she has never recovered her health. She has no appetite, but an actual distaste for food. Vomits almost everything which is introduced into the stomach. The epigastrium is tender on pressure, and she has a good deal of flatulence. She complains of something stuffing up her throat, rendering swallowing difficult, the cause of which is apparently elongated uvula. Emaciation and debility are extreme. Pulse 64, scarcely perceptible; tongue brown; bowels costive.

The patient is evidently weak-minded.

One ounce of castor oil was prescribed, and three ounces of malt spirits daily.

" *7th*, Pulse 96, very feeble; bowels well opened.

A sinapsim was applied over the epigastric region.

The patient was directed to have half a grain of opium three times a day; and the spirit was increased to four ounces in the day.

" *8th*, Remedies continued.

" *9th*, Seems fully easier. No vomiting or diarrhœa; takes a little food.

Remedies continued.

" *10th*, Vomited the oil; no stool; otherwise rather improved.

To have immediately an enema, with half an ounce of oil of turpentine.

" *12th*, Very feeble; pulse 80, small and soft; bowels well opened, stools natural; still continues to vomit occasionally.

To have immediately an opium pill; and if vomiting continues, to be repeated at the hour of rest. Spirit to be continued, and to have three ounces of port wine.

" *13th*, Getting feeble.

Spirit to be increased to six ounces, and wine also to six ounces.

" *14th*, Vomiting less troublesome, and bowels well opened with a clyster last night. Pulse 100, very feeble and small. Takes nothing but her wine and spirits.

Remedies to be continued; and enema to be given in the ening as yesterday.

" 15*th*, Vomited only once. One free stool from injection. lse 96, a little more firm.

Enema to be repeated in the evening. Wine and spirits to be tinued.

" 17*th*, Still excessively feeble. Pulse very small; some wander- ; one small stool.

To have immediately three drachms of castor oil; repeat ene- . in the evening, the scalp to be shaved; and cloths wet with d water to be applied to the head.

" 22*d*, Dead."

The body was inspected on the 24th, when I took the follow- report of the morbid appearances.

Brain.—The membranes were quite natural, but there was nsiderable subarachnoid effusion. The substance of the brain peared healthy, with the exception of two spots about the size a shilling, the one occupying the anterior, and the other the sterior part of the centre of the right hemisphere, which pre- nted the appearance of ramollissement.

Pituitary Gland.—Occupying the *sella turcica* and site of the uitary gland, was a nodulated tumour about the size of a wal- t, which appeared to be an enlargement of the gland itself. It emed to be divided into three pretty distinct nodules, one of iich was very hard and cartilaginous, the others, though hard, re so in a less degree, and the upper surface of the left one emed undergoing a morbid change, being dark-coloured, and in me places broken down. At the upper part of the tumour there peared what seemed to be the infundibulum very much dilated d thickened. The surface of the tumour was smooth and glis- ning, and of a bluish tint, and was attached to the *crura cerebri* d *tuber annulare* by numerous vessels and cellular tissue.

On making a section of the tumour, it appeared composed of ncentric layers enclosed in a cartilaginous cyst, which was en- loped in a covering of condensed cellular tissue. The centre nsisted of soft, broken down, reddish brown, cheesy matter, and the layers approached the surface they became harder, and had bluish gelatinous appearance.

Lungs.—The right lung was firmly adherent all round to the ricardium and parietes of the chest, with which exception both ngs were quite healthy.

Heart.—Attenuated, but otherwise healthy.

Bronchial Tubes.—Situated at the origin of the large bronchial ibes, and covered with a melanotic membrane, were several hard regular masses, which on analysis were proved to consist of car- onate of lime.

Stomach.—The inner membrane of the stomach was very pale, and in some places much stained with bile, but nothing was found to account for the vomiting during life.

Liver.—The liver was light coloured, hard and brittle, and about the natural size; spleen healthy.

Kidneys.—Both kidneys were somewhat smaller than usual. A cyst capable of holding a teaspoonful of fluid occupied the centre of the right kidney. In other respects both kidneys were quite healthy.

In this case the morbid growth was the only example of cancerous disease found in the body. It could not be ascertained whether the patient had ever been subject to fits of any kind, but it was known that she was weak minded.

In more than one of the cases described by the Wenzels the gland was of a scirrhous cartilaginous hardness; and they mention that Petit in a hydrocephalic case found it excessively voluminous, and of a scirrhous consistence.

3. The most singular of all the adventitious productions noticed as occurring in connection with the pituitary gland remains now to be considered. This consists in the effusion of a matter varying from a semifluid consistence to that of a gelatiniform or flaky hardness of various shades of colour, verging from grey to greyish-yellow, yellow, and deep amber. It is generally found underneath the gland and between its lobes. It is described by Rokitansky as "a gummy glutinous (colloid) substance, of a yellow colour, like citron or yellow wine, and occupies the cellular interstice between the two lobes of the gland." The Wenzels describe it as a yellow, viscid, transparent, friable matter, separating the two lobes.

This appears to be the most common morbid appearance in connection with the pituitary gland, at least it has more frequently been found than any other form of disease, and the descriptions given of it are remarkable for their uniformity. The Wenzels found it remarkably distinct in ten of their cases.

Is it an inflammatory product? This is not an improbable supposition, for in several of the Wenzels' cases there was found in the same situation where this substance was deposited, effusions of lymph of various degrees of consistency, and in various transition stages, from that of complete fluidity to that of solid lymph, as well as other traces of inflammatory action. In Duncan's case the inflammatory appearances were abundantly evident, and the amber-coloured body found underneath the pituitary gland formed the central nucleus of a yellow, glutinous, transparent mass lying underneath and between the lobes. Her case exactly corresponded with that of the Wenzels' seventeenth case, where they found between the lobes "a transparent, clear, yellow, and viscid matter, with a central yellow nucleus."

It is I think highly probable that this substance in all the cases related is effused first in the fluid state, and gradually becomes thicker and more consistent, or even solid afterwards. Certainly, if the epileptic symptoms be supposed to be connected with this formation, in all, except the last case related by the Wenzels, they were of sufficient duration for such changes to take place.

As regards the connection of these morbid appearances with epilepsy, it may be stated that the Wenzels incline decidedly to this opinion. They found them remarkably distinct in ten epileptic cases, and in other ten the pituitary gland always presented some morbid lesion. Their observations were made with singular care and minuteness. Idiopathic epilepsy alone was made the object of their study, and they always had a healthy brain beside them, and generally witnesses. On the other hand, Rokitansky says, he has made frequent examinations of the pituitary gland with reference to this question, but has as frequently failed to discern the disease in those who had notoriously suffered from epilepsy and convulsions, as he has met with it in other individuals who had been thoroughly healthy. In the case of Duncan, so often referred to, the epileptic symptoms were distinct, and the morbid appearances described by the Wenzels were equally distinct.

DUNOON, ARGYLLSHIRE,
January 1852.

ART. II.—*Analytical Inquiries, Physiological and Pathological, upon the Spleno-Hepatic Apparatus.* By Dr J. H. BEAU, Physician to the Hospital Saint Antony, Associate of the Faculty of Medicine of Paris. (Archives Generales de Medecine, April 1851, Quatrieme Serie. T. xxv., p. 385.)

PATHOLOGICAL PART.

IT may be naturally expected, that we should make on the subject of the Pathology of the Spleno-Hepatic Apparatus an historical examination, which may be in some degree analogous to that already made on the physiology. This we propose now to undertake.

A great part of the Pathology of Galen rests on his physiological system of the spleno-hepatic apparatus. Thus, besides the proper affections of the liver and the spleen, for instance, obstructions, inflammation, abscess, and similar disorders, Galen refers also the great part of general diseases to these two organs, as the source from which they spring. As the liver is the organ of sanguification,[*] and as sanguification carries with it elimination of yellow bile, which goes to the gall-bladder, and of black bile, which is attracted by the spleen, we ought to find in the augmentation, the diminution or the alteration of these different functions, the cause of a great number of diseases.

In other respects Galen himself fixes his pathogenetical principle in an exact and aphoristical manner. He says that the alterations in sanguification follow the alterations in the liver; *sanguificatio vitiatur hepate vitiato.*[†] For instance, when the liver is hot and moist, it induces plethora.[‡] When, on the contrary, it is cold and dry, the blood is diminished in quantity and the veins are contracted.[§] If the liver be moist and very cold, cachectic diseases and dropsies are the result.[||] Atrophy of the liver carries with it atrophy of the whole body.[¶] When the liver is diseased, it rids itself of its morbid fluids by stool or by the kidneys.[**] Galen distinguishes with much care and importance the diseases which depend on the predominance of yellow bile and of black bile, whether these two fluids are separated from the blood by the liver or issue from their respective reservoirs and thence infect the economy at large. The yellow bile produces all the local and general diseases, in which there is a sensation of burning and inflammation, for instance Rose.[††] On the contrary, black bile engenders cold diseases, especially those of chronic character, as melancholy, madness, apoplexy, convulsions, blindness, carbuncle, cancer and similar maladies.[‡‡] *Veratrium* or hellebore possesses the property of causing the evacuation of this black bile, and consequently of curing the cerebral diseases which depend upon its presence.[§§] Further, yellow jaundice proceeds from the liver; black jaundice from the spleen.[||||]

These form the principal doctrines announced by Galen on the spleno-hepatic apparatus;—doctrines which are dispersed throughout his numerous treatises, and which pervade his whole system of physiology. To this we shall have occasion soon to revert, insisting particularly upon several of these doctrines. With this intention, and with the purpose of observing brevity, I have not supported these doctrines by textual citation.

This doctrine is found entire or in part in the most of the authors who have followed Galen. From the time of this author, indeed, to the middle of the seventeenth century, it is hardly possible to open any treatise on medicine in which mention is not made of the primary importance of the liver in pathology, of the diseases which depend on its different alterations, and of the dileterious influence of the yellow and the black bile. Riolan also in the article of his ANTHROPOGRAPHIA, in which he treats of the liver, remarks that the study of this organ is very important to the physician. 'Sic medici in eo viscere quod vitae et nutricatus fundamentum est, diligenter occupari debent."*

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[*] This organ is also, according to Galen, the principal seat of heat.

[†] Loco citato, Class 4, 33 F. [‡] Isagoge, 65 C. [§] Ibidem.

[||] Sp., 60 B. [¶] Ibid. [**] 4. 34 D. and 35 E.

[††] Ext. 67, E. [‡‡] Ext. 53, D. [§§] 1, 36, D. [||||] Sp. 82, B.

I have said the hepatic pathogeny of Galen prevailed in medical science till the middle of the seventeenth century. It is natural to think that as the *receptaculum* of Pecquet was discovered at this period, namely, in 1649, and as this discovery overturned the physiological theory of Galen upon the hæmatosis of the liver, it may be imagined that the pathogenic corollaries of this hypothesis must of necessity have been destroyed by the same blow.

It is interesting to think that the discovery of an organ so small as the *receptaculum* of Pecquet could have effected so great a revolution in physiology and pathology. Some physicians observing the important reservoir, abjured their old belief; while others continued to defend the traditional foundations of Galenical medicine.

On this subject an interesting fact is recorded by an author of this period, Binninger, physician at Montbeliard. Binninger relates that Pecquet having come to Montpellier at the close of the year 1651, was invited to demonstrate the presence and position of the *receptaculum*, before five professors, among whom was La Riviere or Riverius, as he is often named. Binninger, then only a student, had the honour of being present at this conference, which took place on the 3d of January 1652. After describing at sufficient length the manner in which Pecquet showed in two dogs, the chyle traversing the Receptacle, the thoracic duct, and proceeding to the subclavian vein, Binninger relates that one of the five professors said to Pecquet;—"If matters proceed in the human body, as we have now seen, what is to become of our medical doctrine?"† To this La Riviere answered, "Physicians are philosophers possessing the use of their senses; what they see they believe. *Amicus Plato, amicus Galenus, sed magis amica veritas.* We cannot deny the truth of what we have seen with our eyes." This being said the five professors withdrew in astonishment, after having saluted Pecquet, whom they soon admitted as doctor with all those praises due to his remarkable merit.‡

The reflection of the professor of Montpellier, What is to become of our medical doctrine, must have presented itself to the mind of all those who ascertained, and who admitted that the chyle goes to the subclavian vein without passing by the liver. Bartholin was the first who had the idea of treating this question in its principal points. After having published the writing already mentioned, which contained the funeral and the epitaph of the liver, he maintained the same year at Copenhagen, a Thesis, in which, assuming other subjects, he proposes the question, If the death of the Liver ought to cause changes in medical doctrine?

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* Anthropographia, De Hepate, p. 198. Paris, 1626.

† Si res ita se habet, quid de nostra fiet medicina.

‡ This story is translated almost literally from Binninger. [Johannis Nicolai Binninger Observationum Centuriæ. p. 31. Montbelgardi 1673.]

Bartholin examines first the signs of diseases of the liver, deduced, according to Galen, from excrements become sanguinolent, from the urine, and from the change in the colour of the skin; characters the precise and traditional value of which he calls in question, as indicating rather a disorder of the arterial terminations of the mesentery, of the kidneys, and of the skin, than of the liver. He adverts to the alleged hepatic origin of a great number of diseases, for instance fevers, dropsies, cachectic maladies, atrophies and similar distempers, the origin of which he places in the heart and not in the liver. Lastly, having come to the therapeutic department, he recommends that we should employ against affections of the liver, the means shown by experience to be efficacious. It is in all instances necessary to endeavour to disperse obstructions, to soften skirrhous affections (indurations), to allay inflammations, to correct the excessive heat of the liver, and to restore the organ to its normal condition; not because sanguification takes place in the liver, but because the blood, which proceeds to the liver, then traverses the organ with greater difficulty, and no longer furnishes the serous portion, which should return by the lymphatic vessels. Lastly, Bartholin terminates his dissertation by the following propositions, which contain the summary recapitulation of the doctrines which he promulgates.

1. That the ancient and received method of treatment is not changed, but merely receives elucidation.

2. That the causes of diseases become more clear and approach more nearly to truth by reason of these new discoveries.

3. That diseases of the blood may depend on obstructions and other maladies of the liver, though this organ be not concerned in sanguification.

4. That nothing of the blood from the liver is derived to the mesentery, according to the principles of the circulation, nor does any chyle come to the liver, according to the observations upon our lymphatic vessels.

5. That the authority of the ancients is in no instance to be preferred to experiment of the most certain kind.

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* Dubia de lacteis thoracis, et an hepatis funus immutet medendi methodum. Hafniae, 1653.

6. That in the treatment of all disorders, attention ought also to be given to the heart.*

To this attack upon the medical doctrine of Galen, Riolan failed not to give an answer.† But Bartholin quickly after made a rejoinder, in which he maintains all the propositions of the preceding examination.‡

Bartholin, as may be observed, in this examination of the pathological phenomena which he wishes to withdraw from the liver, introduces very happily the Harveian discovery of the circulation, and he substitutes with ability the heart for the liver. In other respects, it must be allowed, that the discovery of the circulation, then recently made, comes to give a powerful support to that of the Receptacle of Pecquet, in overturning the hepatic hypothesis of Galen; for, as I have already said, the function of haematosis had been withdrawn from the liver and assigned to the lung.

There was accordingly a certain bond of union between the discovery of Harvey and that of Pecquet, in order to make a successful opposition against the Galenical doctrine. And, as sometimes takes place, when great scientific truths are the subject of inquiry, the discussion extended from the medical world, and excited the attention of the public at large, some of whom became more or less strongly roused against the long established ideas of the medicine of the ancients. These ideas were even brought upon the stage, and exposed to the derision of the world at large.§

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* 1. Methodum antiquam receptamque medendi non immutari sed illustrari.
2. Causas morborum novis his inventis veriores clarioresque evadere.
3. Ab hepatis obstructionibus aliisque affectibus, quanquam non sanguificet, sanguinis vitia dependere.
4. Nihil sanguinis ex hepate ad mesenterium derivari, ex circulationis principiis, nec ullum chylum ad hepar pervenire, ex observationibus nostrum vasorum lymphaticorum.
5. Veterum auctoritatem experientiae certissimae nunquam esse praeponendam.
6. Cordis, in omnium affectuum curatione, habendam quoque esse rationem.

† Riolani Responsiones duae, et judicium novum de venis lacteis. Parisiis, 1655.

‡ Bartholini Defensio variorum lacteorum adversus J. Riolanum. P. 151. Hafniae, 1655.

§ Of this reaction Moliere was the most powerful instigator. His satirical representations of medicine and physicians are written in the classical terms of the language of Galen; and his explanations of burlesque pathogeny rest almost always upon the alleged agency of the liver or the spleen. Thus in one part he speaks of the hypochondriac melancholy which proceeds from disease of the spleen, the heat and inflammation of which carry to the brain many thick, gross, fuliginous, or sooty vapours. He specifies the symptoms indicating a fuliginous and pungent vapour, which irritates the membranes of the brain;—or further, of malignant vapours engendered in the concavity of the diaphragm. Again, in speaking of a dropsical female, he says that "her liver, her belly, or her spleen, in place of making blood, make only water." In another sense arises a question of an intemperies of the

Physicians accordingly substituted in place of the theory of Galen upon the liver and its functions, the discoveries made in the seventeenth century; and by these they believed that it became more easy to comprehend the phenomena of life and the causes of diseases. Among the latter subjects, there is in particular one disease, the pathogeny of which appears quite simple and natural according to the new doctrines. This is dropsy. The formation of this disease, Galen, it is well known, explained by the assumption of a cold and moist state of the liver. Several physicians believed that the cause of this disease consisted in rupture of the lymphatic vessels and effusion of lymph. Lower meanwhile opposed this manner of explanation, as one which is purely imaginary. He was the first who demonstrated the proposition, that obstruction of the veins is capable of producing dropsy; and this he did by the aid of a decisive experiment, that of enclosing the venous trunks in a ligature.*

Further, one hundred years after the time of Lower, Van Swieten, commenting upon this experimental fact of Lower, remarks, that the ancients had some reason in regarding the diseased liver as the cause of all dropsies; because this organ, when it is changed by disease, must of necessity obstruct the venous circulation, either of the *vena cava*, which traverses it, or of the portal vein, which

splenic parenchyma, that is to say, of the spleen or of the liver; for parenchyma means both the one and the other organ, by reason of the close sympathy which subsists between them through the medium of the *vas breve* of the *pylorus*.

His physicians, as thus strongly imbued with Galenical doctrines, Moliere represents, in order to cover them with ridicule, as strictly opposed to the doctrines of Harvey and of Pecquet. "I have," says one, "maintained a thesis against the Circulators." The same person, at the saying of his father, who is like himself an obstinate partisan of the ancient medicine, "is blindly attached to the opinions of the ancients, and he never desired either to listen to, or to comprehend the reasons and the experiments of the alleged discoveries of our age (the seventeenth century) regarding the circulation of the blood and the other opinions of the same stamp;" that is to say, the opinions of Pecquet and of Bartholin, which, as has been above said, constituted part of the same body with that of Harvey.

By this it appears that Moliere attacked medicine in general less than is commonly believed. He merely took part in the discussion, which was at that time going on, between the Galenical physicians on the one hand, represented by Riolan, and Bartholin on the other side, who was maintaining the new ideas. He had reason to attack Galenism, in whatever false and ridiculous doctrines it really contained, and especially the black bile of the spleen and its supposed deleterious influence.

It is well known that Moliere received his information as to the facts and the style of the medicine of the day from Mauvilain, professor of the faculty.

* Richard Lower is one of the most original and the most acute authors of the seventeenth century. He has, in particular, shown all the pathological consequences of the discovery of Harvey, in a small work of about two hundred pages, full of facts and judicious reflections (Tractatus de Corde Amstelodami, 1671). In this author we find a theory of the uses of the pancreatic juice, which approaches in some degree to that given by M. Bernard—"Cum autem chylus per hujusmodi poros et angustias ex intestinis in lacteas transeat, a ratione non alienum videtur, ab humore e pancreate in duodenum secreto, pro faciliore transitu magis dilui, et attenuari." Ibid. p. 202.

distributed through its substance. “ Nec mirum est, veteres par male affectum accusasse tanquam omnis hydropis causam, m toties male affectum hoc viscus in hydropicorum cadaveribus veniatur, cava ascendens per hepar transeat, et vena Portarum r totum hujus visceris substantiam distribuatur.”*

If ever then a cause of disease was properly demonstrated, it is undoubtedly the obstruction of the veins in the production of opsy. This cause, nevertheless, was gradually thrown aside, d it was so thoroughly forgotten at the commencement of the esent century, that M. Bouillaud, having a second time disvered the fact made known by Lower, taught physicians that opsies depended on the impermeability of the venous trunks.†

But it behoves us at this point to return once more to the at discussion of the seventeenth century, in order to show, that, mediately after it was terminated, there arose a powerful meal school, which endeavoured to cause the revival once more the opinions of Galen upon the absorption of alimentary articles the *vena portae.* This was the school of George Ernest ahl, Professor at Halle, during the end of the seventeenth and ginning of the eighteenth century. He died in 1734 at the of seventy-five. The essay, *De Vená Portae, Portá Malon*, was published in the year 1698.

It is well known that this pathologist had founded a part of his tem upon the functions and affections of the portal vein, ich, in the opinion of Stahl, was the source of a great number diseases. This idea he had expanded and elucidated in a distation often quoted, as much for its doctrinal importance as for singularity of its title;—*De Vená Portae, Portá Malorum pochondriaco-Splenitico-suffocativo-hysterico-colico-haemorrdariorum. Halae*, 1698.‡

This dissertation, which is a curious monography on the affecns of the Portal Vein, consists of an introduction and anatoal, physiological, pathological, and therapeutic divisions. The st important points are the following.

n the physiological part, Stahl speaks of the discovery of the eal vessels as a circumstance, which had contributed to make sicians lose sight of the functions of the Portal Vein; and ecals the controversy which had taken place on this subject, tioning particularly the Hollander De Bils, who performed t a part so important “ At vero recentiores sub primum ultum et altercationes circa vasa lactea et circulationem sanis deprehensam, excludere venam portæ, et propagines ejus iraicas ab omni commercio chyli ejusque transmissione sa-

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Van Swieten Commentaria, T. iv. p. 160. Paris, 1775.

See Note (A) at end of the Memoir.

Haller Disputationes Anatomicae, T. iii., p. 131.

tagebant. Unde Bilsianæ illæ, et anti-Bilsianæ demonstrationes, quas vel allegare, nedum recensere, longum sit."* (146).

Stahl adopts upon this subject a mixed opinion, and regards the absorption of chylous matters as taking place all at once by means of the lacteal vessels and the portal vein. "Interim," he says, "ubi primus impetus resedit, et respirare a partium studio, licuit curiosis, per varia experimenta salis deprehensum esse apparit, quid utique per venas mesaraicas, haud multo minus chyli imbibatur et cum sanguine ad hepar feratur, quam per ipsas ita dictas lacteas venas." (147).

In the pathological part of his dissertation, Stahl explains the alterations incident to the *Venæ Portæ*. Of these he distinguishes four as the principal; 1*st*, Lesions in the capacity of the Portal Vein: 2*d*, Lesions in the consistence of the blood which flows through it, which are themselves dependent on different causes, and especially when the introduction of *ingesta* into the portal vein, when they are, as he represents, acid, feculent, and *mucid -crassa*. (p. 161); 3*d*, the passive lesions of the movement of the blood; and 4*th*, the active lesions of the movement of the organs attached to the Portal Vein. (p. 155). In these different lesions Stahl finds the causes of Hypochondriasis, of Hysteria, of Obstructions of the Spleen, of Hemorrhoidal Colics, of Hæmatemesis, of Askites, of Jaundice, and similar disorders. In short, as the title of this dissertation indicates, the *Vena Porta* is, in the opinion of Stahl, the gate or channel for the admission of a great number of diseases.

Stahl, as may be seen, avails himself of the hypothesis only in order to explain the inspissation or thickening of the blood of the Portal Vein induced by the introduction of certain *ingesta* into its blood. This inspissation is certainly possible, but it is far from being demonstrated. Stahl urges his researches no further. He does not ask if there be not in the liver certain affections which are more truly dependent on the *ingesta* than the thickening of the blood of the Portal Vein. As to alterations of the sanguifying power of the liver, of these no mention is made.

This last relic of the hypothesis of Galen gradually disappeared with the theories of Stahl, more particularly when Hunter had enunciated the proposition that the lymphatic vessels were the exclusive agents of alimentary absorption.

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* "But indeed the [illegible] on the first disturbance and controversies concerning the lacteal vessels and the discovery of the circulation of the blood, had quite enough to do in examining the Portal Vein and its mesenteric branches with [illegible] and its transmission. Hence arose those demonstrations of the Bils and against that person, which it would be tedious to enumerate, and much more so to examine."

In order to terminate this historical sketch, and to show what was at the commencement of the present century the state of physiological opinion upon the subject now under consideration, I shall adduce a passage derived from the introduction of the work of Portal upon the Diseases of the Liver,—a work which was for a long time classical. "They," says Portal, that is, "the ancients, had imagined that the liver was the organ of sanguification, the source of animal heat, the seat of the natural faculties, &c., &c., and, consequently, from their erroneous theory, they formed very fallacious notions upon the nature of the diseases of this organ, and often upon the treatment required for these diseases."*

Meanwhile, considering that, contrary to the opinion of Portal, we have returned to the physiological ideas of the ancients, excepting the modifications indicated in the first part of this Memoir, it is absolutely necessary that, with these physiological notices, the pathology should be in harmony. This relation it is our object to establish, taking, nevertheless, for our guide, the actual observations made on patients.

The first question to be fixed is that of ascertaining whether the substances ingested, which are absorbed by the rootlets of the Portal Vein, and which penetrate through this Vein to the liver, may not, in certain instances in which they are of an irritating nature, determine directly by their contact, morbid affections in the organs which they traverse. It will afterwards be seen that facts authorise us to decide this question in the affirmative. Theory, in other respects, indicates beforehand the possibility of these affections. It proceeds even further. It leads us to propose the question, by what means it happens that substances extremely irritant, for instance alkohol, certain seasonings, and similar articles, of which many individuals make use, can traverse the Portal Vein and the liver without habitually exercising a hurtful influence upon those delicate and irritable organs?

To this question the answer must be given, that the *vena portae* and the liver, having to undergo the contact of substances conveyed into the special black blood which traverses these organs, have been endowed with a certain physiological resistance to the irritant action of these articles. The *parietes* of the Portal Vein are a great deal less irritable than those of general venous system. In respect of irritability, the Portal Vein differs from the *Venae Cava*, as the mucous membrane of the alimentary passages differs from the mucous membrane of the respiratory tubes or the genito-urinary organs. Now we know that certain substances which produce a feeble impression upon the *parietes* of the mouth, of the pharynx, of the œsophagus, would determine an actual irritation, if they were made to penetrate into the canal of the urethra, or the nasal cavities.

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* Observations sur les Maladies Du Foie. Introduction, p. 5. Paris, 1813.

This resisting power of the Portal Vein, then, and of the liver. may be destroyed under the influence of different causes. And in that case substances, which hitherto had not produced upon the liver any injurious influence, may affect this organ, and may at length effect in its structure serious changes.

Among the causes which thus dispose the liver to be affected under the influence of certain ingesta, pathologists are in the habit of enumerating sedentary habits, prolonged mental chagrin, night-watching, the labours of the closet, anxiety, and particularly residence in certain hot climates, as those of India, of Africa, and of America.

It is a task of sufficient difficulty to say in what manner the causes now specified act in giving the liver a pathogenic disposition. This result they may produce in two modes. They may act either by removing from the liver its power of physiological resistance, as has been already said; or by diminishing the assimilating faculty in the Porto-splenic blood to such a degree, that the substances absorbed being no longer sufficiently modified and assimilated in the blood which conveys them, arrive at the liver without having lost their nature, which may be very irritating.

We would have to study the influence of the *ingesta* both upon the Portal Vein and upon the liver. But the affections of the Portal Vein being imperfectly understood, even since the publication of the celebrated dissertation of Stahl, we have nothing at present to add to the little that is known upon these affections. This is a subject for interesting research, upon which almost every thing is yet to be done, and which we suggest to the attention of observers. At present, I am to occupy myself exclusively with the liver, beginning with the disorders known under the name of hepatic colics.

HEPATIC COLIC.

These attacks consist in pains, more or less violent, transitory, and intermittent, which have their seat in the liver.

Ordinarily they are preceded, for a long time before their appearance, with a fleeting sensation of weight, tightness, and uneasiness in the right hypochondriac region. Ordinarily, also, these precursory phenomena correspond with slight hypertrophy of the liver, which protrudes in a sensible manner beyond the false ribs, and they are accompanied with constipation.

The pain of hepatic colic has the same form as that of stitch in the side, which is felt in the right hypochondre, when the individual runs, immediately after having taken a meal, but it frequently acquires considerable intensity. The patient feels as if the liver were locked within a case. He gives evidence of the pain which he suffers by sighs, complaints, and even by cries; his face is pale, the features altered. He is agitated, continually changes his position, with the intention of alleviating his sufferings.

This pain is sensibly aggravated by pressure. It is sufficient in several cases to apply the finger upon the portion of liver which protrudes, or even upon the ribs which cover the liver, to show that this simple contact cannot be endured by the patient.

The pain may be uniformily diffused throughout the whole liver; in other instances it has circumscribed seats of greater intensity. In this manner patients complain of it as being in three different places; 1*st*, Most frequently in the epigastric region; 2*d*, In the middle part of the right hypochondriac region; 3*d*, In the posterior part of the three or four last ribs. But in the cases in which it is predominant at the spots now specified, it affects, nevertheless, the liver in its entire extent. Of this fact it is easy to obtain evidence by the aid of pressure and especially of percussion.

With the pain is associated swelling, or more or less enlargement of the liver. Of this it is easy to form an estimate, by the aid of palpation, over the whole portion of the liver which protrudes. It is on this point only that swelling was observed first by Tacconi,* and afterwards by Pujol, Soemmering, and others. But the existence of the symptom may be established also over the whole organ by means of comparative percussion practised during and after an attack of colic.

This swelling is further perceptible by the patient, who has the sensation as of an enormous mass which fills up the right hypochondriac region.

From the swelling and the pain in the liver it results that the diaphragm is remarkably constrained in its movements of depression. Hence proceeds the breathlessness (*dyspnœa*) peculiar to hepatic colic, breathlessness, of which the character consists in short, numerous, interrupted respirations.

All authors have remarked, as a habitual symptom of hepatic colic, vomitings of bilious matters. They have also observed that the pulse possesses nothing febrile, even during the presence of the most acute pains.

These form the principal symptoms of hepatic colic;—pain and swelling of the liver, breathlessness, bilious vomitings, and the pulse void of any febrile character.

The approach of hepatic colic is almost always announced by a sensation of constraint or tightness, swelling, and weight in the right hypochondriac region. The duration of this preliminary symptom may be some minutes, some hours, and even one or two days. Some seconds then only are requisite in order that the sensation of weight may augment and proceed to the state of pain. It is then that colic commences.

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* Tacconius de Raris Hepatis Morbis. Bononiæ, 1741.

The duration of hepatic colic, properly so named, is half an hour at the least, and at the most two or three days, during the course of which there may be several exacerbations. The cessation of the attack is altogether analogous to the approach and commencement; that is to say, that if the precursors have lasted two or three days, there will then be uneasiness and swelling of the hypochondriac region for two or three days after the cessation of the pain properly so called. If, on the contrary, the advent takes place suddenly, and in a manner as it were instantaneous, the same will be the mode of the cessation of the colic.

The pain which distinguishes hepatic colic diminishes not insensibly. It disappears all at once in four or five seconds, at the moment when it is most considerable, and when the patient thinks himself condemned to undergo it for a long time. The patient then makes two or three deep inspirations in order to relieve himself in the most speedy manner of the constraint which he feels in breathing. Dating from this moment all the symptoms have ceased, the physician may practise palpation and percussion on the liver without causing uneasiness. There is left only a slight sensation of bruising and itching.

One very apparent symptom which takes place after the cessation of the attack, or before the end of the attack, when the latter has been lengthened, is Jaundice. The jaundice lasts in general as long as the colic by which it has been induced. But it takes place not in all instances, even when the attack of colic has been violent. It may be wanting in hepatic colic, as it is sometimes wanting, without our knowing why, in attacks of *kirrhosis* marked in the most distinct manner.

As to the nature of hepatic colic, that must be sought for in the consideration of the nature of the pain, which is acute, considerable, intermittent, without fever, sudden in its invasion and in its cessation ; which, in one word, possesses all the characters of visceral neuralgic attacks. Hepatic colic is therefore a HEPATALGIA. It remains to be learned if this hepatalgia is idiopathic or symptomatic. This point is now to be examined.

We may very well arrange with the neuralgic character of the colic, the swelling of the liver, which always commences with the pain, and which is proportional in intensity with it. The swelling arises from the influx of blood induced by the pain itself. *Ubi dolor*, *ibi fluxus*. We know, that in neuralgic attacks of the ophthalmic branch, the eye is red and injected during the neuralgic paroxysm, as it is in an attack of ophthalmia. The same is the case with the liver affected by neuralgia. Fluxion, that is the influx of blood, is so much more easy and intense, as the texture of the liver abounds in blood-vessels. This orgasm further proceeds the length of augmenting the volume of the liver. In short, it produces swelling.

Hepatalgia is often confounded with other painful disorders, so long as jaundice does not make its appearance. The mistake is so much more easily made, that the hepatic pain may, as has been said, have a seat of greater intensity in the epigastrium, in the middle part of the hypochondrium, or at the level of the posterior third of the lower ribs; and in these three places the hepatic pain may be taken for a pain of gastralgia, of intestinal colic, or of nephritic colic.

Percussion will obviate any mistake on this point, even before the manifestation of *icterus*, and will show that the affection is exclusively seated in the liver, by permiting the physician to ascertain the following points; 1*st*, That the liver is altogether enlarged in volume; 2*d*, That all the dulness circumscribed by the volume of the liver, before, behind, and upon the side, is extremely painful on percussion, whatever be the place circumscribed in which the spontaneous pain is most acutely felt.

We proceed to the causes of *Hepatalgia*. According to the greater number of physicians, hepatic colic is caused solely by a gall-stone which temporarily obstructing the *ductus choledochus* produces a transitory and painful retention of the bile in the liver and within the gall-bladder; so that the terms *hepatic colic* and calculous colic are entirely synonymous.

What has led physicians to understand in this manner the cause of all hepatic colics is a series of reasons which I shall now state.

1. The first is because physicians have often occasion to ascertain the presence of gall-stones more or less numerous in the gall-bladder in the bodies of persons dead of different diseases.

This first reason is not valid, though the fact of the frequency of gall-stones be true. At the Salpetriere few inspections of the bodies of aged females are made, without meeting in the gall-bladder a number more or less considerable of gall-stones; and, nevertheless, hepatic colics are extremely rare at the Salpetriere. I did not observe a single instance of this affection during four years which I spent in the hospital as pupil, and as head of the medical body.*

2. It has appeared to physicians very natural to explain the sudden attack of colic and its sudden cessation by supposing that

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* I am happy that I have it in my power to confirm this result of my own observation by the experience of M. Rostan, who informed me that the impression which he retained on this subject of his long residence at the Salpetriere was the great frequency of gall-stones in the inspections, and the extreme rarity of attacks of hepatic colic.

a gall-stone came instantaneously and obstructed the bile-duct, and that it in like manner ceased instantaneously to interrupt the course of the bile.

On this point it may be said that certain neuralgic attacks, for instance those of *angina pectoris*, begin and terminate suddenly, though it is not necessary, in order to explain these phenomena, to have recourse to the supposition of a mechanical displacement.

3. The swelling of the projecting portion of the liver, remarked by Tacconi, Pujol, and others, has been further considered as the result of mechanical distension of the gall-bladder and the biliary passages dilated by the bile which the calculous obstruction prevents from flowing into the intestine. "This swelling," says Soemmering, "disappears after the discharge of the gall-stones."

It is known that the swelling is not confined only to the protruding portion of the liver, but it affects the entire mass of the gland; of this it is easy to be satisfied by the aid of percussion. Consequently this swelling depends not upon the distension of the gall-bladder, but on the influx of blood which affects the whole liver, and which disappears with the pain, when the attack of colic has ceased.

4. The analogy derived from nephritic calculous colic, or that from the presence of stone in the kidneys, has principally contributed to confirm the idea that colic of the liver depends in like manner on the presence of a gall-stone.

There are, however, in the symptoms of these two affections differences which do not allow us to view their cause in the same manner. Thus so long as nephritic colic lasts, the urine is suppressed. This is perfectly natural because the urinary passages are obstructed by a concretion. On the contrary, in hepatic colic and during the presence of the most marked symptoms, there are frequently bilious vomitings. Whence, it may be asked, proceeds this bile, if the biliary duct is actually obstructed by a gall stone. When nephritic colic has terminated the patient discharges with the urine the concretion or concretions which had caused the urinary obstruction and the pain in the kidney.* In hepatic colic nothing is so rare as the discharge of a gall-stone after the cessation of the pain. M. Chomel, a clinical observer of great experience, maintains with a great deal of reason, that it is the most if we observe this result in one instance among thirty or forty cases of hepatic colic.

From all that has been now said, the conclusion must be, that the inference rests not on a sufficiently solid foundation, that every attack of hepatic colic has necessarily its origin from the resence of gall stone.[*] We cannot indeed deny the existence hepatalgic attacks produced by a gall-stone ; but these are not mmon. In conclusion, it must be admitted, that, in the great ajority of cases, neuralgic attacks in the liver proceed from other uses than calculous obstruction.

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* This proposition must not be absolutely expressed. Sometimes there are nephritic colics which do not depend on calculous obstruction.

What then, it may be asked, are the causes of hepatalgia other an the presence of a gall-stone in the biliary passages?

It may be said *a priori*, that these are all the causes of visceral uralgias in general. In this sense it is necessary to make rearches in order to elucidate the etiology of hepatic colic; I ubt not that there are observed syphilitic, gouty, and rheumatic patalgias. One of the last species I have already met with. he individual in whom it took place, who was subject to rheuatic ailments, was without doubt affected by hepatic colic, which as distinctly marked, whenever he took not care to clothe himlf warmly, especially at the period of the first cold weather; and ring the whole time that the acute or dull pains in the liver ntinued he was exempt from every other species of rheumatic sorder.

There is one cause of hepatalgia, which to me appears to deserve rticular attention, as connecting itself in an intimate manner ith the subject of this memoir. It is the cause which resides in rtain articles ingested, and absorbed by the Portal Vein, and nveyed to the liver, in which they directly excite neuralgia.

I have had occasion for four or five years to observe some amples of hepatic colic arising from this sort of cause. I shall late them successively, enumerating only those details which are ost important.

1. M. X., affected for several years with hypertrophy of the ver, with projection of this organ below the false ribs of the ght side, was subject at the same time to a painful sensation of eight, load, and swelling in the right hypochondriac region; hich sensation followed regularly after taking food, and lasted ree or four hours. In the course of February 1846, this sympm was progressively aggravated. The bowels were constipated.

On the 22d of the same month, M. X. took among other rticles to breakfast, baked apples, something acid; and some inutes after eating the fruit, he felt all at once a sensation of urning heat, which spread from the epigastrium into the whole ght hypochondriac region. This was immediately followed by ainful tension in the part of the liver which extended beyond the ribs. This tension lasted only about twenty minutes, and it was then succeeded by a sense of constraint and load which was, so to speak, continual for the space of one month.

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[*] M. Andral has for a long time been opposed to this as an exclusive cause of epatic colic. "We observe sometimes," he says, "in the hepatic region, pains xtremely acute, which cannot be explained, after death, by any lesions in the ver, or in its excretory ducts. This forms the case of certain hepatic colics."—linique Medicale, Tome. iv., p 194. Paris, 1831, 8vo.

In the evening, M. X. began dinner with sufficient appetite. After soup, he drank two glasses of Madeira; but four or five minutes afterwards, he experienced anew, as in the morning, a burning sensation which extended from the epigastrium into the whole right hypochondriac region, where it was followed by considerable painful tension, with difficulty in breathing. M. X. was obliged to leave the table. He went out and endeavoured to walk strenuously, in order to dissipate the great pain which he suffered in the right hypochondriac region. At the end of one hour and a half it became more endurable. M. X. returned to the house, went to bed at ten o'clock, and prepared to take a bottle of Pullna water, in order to obviate constipation, which he regarded as the cause of the symptoms. But some minutes after having swallowed the first glass, the liver became once more the seat of considerable painful tension, more intense than ever. Respiration was performed with anxiety, interruptions and sighs. The patient changed his position every moment. He brought up by vomiting, first the glass of Pullna water, and afterwards yellow bile. The pain was so violent in the right hypochondriac region, that the slightest pressure could not be borne, not merely upon the projecting portion of the liver, but also upon the costal wall by which the organ is covered. Percussion showed that the whole liver was increased in volume. There was neither thirst nor frequency of pulse.

These symptoms continued at much the same rate all night, the whole of the day of the 23d, and diminished only on the 24th. On the evening of the 23d, jaundice made its appearance, and increased to the 25th. The patient did nothing but drink solution of barley water syrup, and apply cataplasms over the hypochondriac region.

On the 25th, the patient was able to rise. Some pain and swelling of the right hypochondriac region remained, but greatly less than formerly. The amendment proceeded daily increasing; the jaundice also disappeared; but the patient remarked that whenever he took undiluted wine or acid condiments, he felt in the site of the liver, an aggravation of dull pain, which lasted during the whole time of digestion. Upon one occasion among others, after having eaten radishes which had a strong taste, he felt a violent pain in the right hypochondriac region with swelling and breathlessness, a genuine attack of colic, in short, which lasted three hours, and suddenly ceased in the course of some seconds.

Two years afterwards, other attacks of colic ensued under the manifest influence of the *ingesta.* In May 1848, the liver once more became the seat of constriction and weight at each period of digestion. Notwithstanding this warning, M. X. took one day at dinner sallad dressed with vinegar. He rose from table with a sensation of load considerably greater than the previous days. This sensation gradually increased, then all at once in the night it was converted into colic, properly so called, with pain, swelling, breathlessness, moaning, &c., which colic lasted till morning, and all at once ceased at 7 o'clock. M. X. rose as usual, attended to his ordinary occupations, having in the right hypochondriac region only a trifling feeling of pain left. In the afternoon he took a glass of natural Vichy Water, (the Hospital spring), according to advice which was given him. But some minutes only after taking this water, the pain, swelling, and other symptoms, returned quite as intensely as during the night, lasted three hours, and suddenly ceased in some seconds. After the second occasion, the urine was strongly coloured yellow and turbid. Next morning appeared slight jaundice, which receded in the evening.

During the same year other attacks took place, always under the influence of *ingesta.* M. X. resolved at length to go to Vichy. The waters were well endured, and greatly diminished this painful susceptibility of the liver. It is requisite to add, that, after these numerous attacks, very different in intensity, no gall-stone was at any time voided.

2. In the month of April 1846, there came under my care at the Hotel-Dieu-Annexe,* where I then was, a woman, aged twenty-six years, who informed me that she had been ill with *hepatitis.* This medical term she had learned in another hospital in Paris, where she had remained several weeks before entering that of the Hotel-Dieu-Annexe.

In point of fact, this woman herself made the attendants observe the liver, which protruded from four to five centimetres beyond the right false ribs, from the epigastrium to the iliac region. The hepatic protuberance was so much the more easily felt, that the abdominal muscles were relaxed and attenuated in consequence of previous pregnancies. The liver was moderately painful on pressure, not only on the prominent part, but also in its whole extent. When it was attempted to circumscribe this painful sensation by means of percussion, the patient made a gesture indicative of pain, as soon as the examiner proceeded from the sonorous portions to the part where the sound was dull.

This patient stated that this pain became sometimes intolerable, and that at the same time the liver was enlarged in volume. This took place most certainly at all the times at which she took undiluted wine, fruits, articles of food salted, peppered, or spiced. The pain began about a quarter of an hour after the ingestion of these different substances, and lasted for five or six hours. She never had any attack of jaundice.

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* An additional part of the Hotel-Dieu for the accommodation of patients.

This patient was put on the use of poultices over the region of the liver, baths, gum-water, and pastilles of Vichy.* She took two meals daily. She remained in the hospital twelve days, and then left it remarkably relieved.

3. On the 29th August 1846, there came under my care in the Hotel-Dieu-Annexe, a man aged sixty years, who had moderate hypertrophy of the spleen owing probably to a marsh-miasmatic disorder contracted ten years previous at Rochefort, where this man was affected by sundry attacks of intermittent fever. This, however, was not the cause which brought him to the hospital; he came thither for the removal of pains, under which he had laboured for the space of fifteen days, in the right hypochondriac region.

The sensation of pain in the right hypochondre is continued; but it is greatly aggravated after meals, especially when the patient takes undiluted wine and spiced dishes. The pain is then violent; the whole right side is, he says, swelled. At the same time the respiration is very much oppressed. Then at the end of four or five hours all these symptoms disappear almost at once. He has at no time had jaundice.

The patient when examined presents a hypertrophy of the liver, which extrudes beyond the ribs two inches. This organ is slightly painful, not only in this part, but throughout its whole extent, as is proved by the aid of percussion. The patient has no fever, but the appetite is gone, the mouth is pasty, and the tongue is covered with a white fur.

The patient was placed on the use of gum-water. A poultice was applied over the right hypochondriac region; leeches were applied to the anus; baths were prescribed.

On the 1st of September the appetite had returned, and less pain was felt in the region of the liver. Soup was allowed twice daily.

On the 2d, the state was the same; but the patient complained that afterwards transitory pains were felt in the liver.

On the 3d, the appetite was increased. Food was allowed without wine.

On the 5th, the patient had some undiluted wine. Some minutes afterwards violent pains were felt in the right hypochondre, with swelling and breathlessness. These symptoms lasted three hours.

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* The pastilles and lozenges of Vichy consist of nearly pure bicarbonate of soda.

On the 6th, the appetite was increasing. Food without wine was allowed.

On the 10th, two meals were allowed, and wine diluted with much water. This was well borne.

The patient proceeded to convalescence. He left the hospital on the 20th, no longer complaining of pain in the liver, even on pressure or percussion. This organ, nevertheless, projected sensibly beyond the ribs.

4. In the course of April 1846 I was called in consultation in the street Beaubourg, to a young workman of about twenty-four years, of good constitution, who for the space of two days had suffered from violent colic pains, and who since the previous evening had a well-marked attack of jaundice. He was then suffering the most acute pains, in great agitation, and with the respiration interrupted.

His wife, who seemed intelligent, informed me that her husband, though in habitual good health, complained often, after taking food, of dull pain in the right side; which she ascribed to the excessive taste of her husband for mustard. For some time he had experienced adverse circumstances; and with these, the dull pains of the right side had increased. At last, two days ago, after dinner, at which he had taken his usual dose of mustard, he felt no longer a dull pain, but a violent colic, which lasted since that time. At first, he brought up by vomiting the food ingested, and then yellow bile.

Upon examining the patient, I found a firm resistance under the right false ribs; the slightest pressure was extremely painful. Percussion denoted increased size of the liver; as soon as we passed from the sonorous parts to the seat of the hepatic dulness, the patient complained of acute pain. The point to which the patient referred the most violent spontaneous pain was the posterior part of the three last false ribs on the right side; the heat of the skin was natural; there was scarcely any thirst; the pulse was 76.

The patient was placed in a long-continued warm bath, which was repeated next day. In the interval between the two baths leeches were applied to the anus. Ptisan of sweetened gum was given.

I learned from the ordinary medical attendant, that after the application of the leeches the pain was abated. Three days afterwards it was so slight, that the patient could quit his bed. At the same time the jaundice disappeared, and gradually the patient took food as usual.

5. On the 30th of May 1848, I observed at the hospital Saint Antoine, the case of a female sufficiently similar to the one

mentioned in case 2d. This woman, thirty years of age, and who had borne several children, was labouring for about twelve months under the disease for which she had entered the hospital. The symptoms were permanent though slight pains in the right hypochondriac region. These were aggravated to an intolerable degree each time that she took undiluted wine and acid articles. The pains were then accompanied with swelling of the whole hypochondriac region, with moaning, cries, and hiccup.

The liver was hypertrophied and projected beyond the ribs by three centimetres. It was painful on pressure not only in this part, but over the whole extent of the dull-sounding portion.

The patient had some appetite. The pulse was without any febrile reaction.

The patient was placed on the use of alkaline baths, diluted Vichy water, and poultices over the region of the liver. She took two meals daily. She left the hospital at the end of ten days nearly in the same state in which she was when she was admitted. She had at no time any jaundice.

6. The details of this case were given me by the patient and his physician. M. X., residing in the Provinces, of strong constitution, aged about fifty years, presented in the winter of 1846-1847, all the symptoms of hepatic colic, such as excruciating pain in the right side, absence of fever, duration of pain during six or seven hours. This patient was sent to Vichy in 1847. The use of the waters was to him most beneficial.

M. X. always had the attacks of colic after dinner, especially when, dining with friends, he drank a little more wine than ordinary. The susceptibility of the liver became so great, that for one month he could not take the ordinary sauces moderately spiced, wine diluted with water, baked fruits, and similar articles, without suffering immediately afterwards pains more or less acute in the hepatic region. He was obliged during this period to subsist upon soups prepared from milk, sauces, and white meats, and to drink only water.

7. In 1849, being judge of the competition for the place of physician at the Central Board, I had occasion, among the numerous patients examined for the purpose of Clinical Lecture, to meet with two cases of hepatic colic. They were two men, of about thirty years of age, placed in the hospital of La Charité. Neither of them had ever had any attack of jaundice; but in both there was hypertrophy of the liver with slight prominence of this organ below the false ribs, and when the outline of the organ was limited by means of percussion, a sensation of pain, remarkable at the instant at which the transition was made from the sonorous to the dull sounding parts, was perceived by the patient. Both patients had an increase of pain in the hepatic region during the whole time of digestion; and this pain became agonizing when they drank undiluted wine, or took acid aliments.

8 A person, aged sixty-four years, an old soldier, of strong constitution, experienced, from time to time since 1810, pains more or less violent in the right hypochondriac region, without its being possible to say exactly whether they were or were not those of hepatic colic; this, nevertheless, was probable, if attention is given to what took place subsequently. This man was strongly attached to the use of red wine; and rather often it happened to him to drink four or five *litres* of this liquor in the course of one day, without eating, yet without ever presenting notable symptoms of intoxication.

On the 27th of June 1850, he spent the day in libations with a friend, and drank his habitual allowance of five *litres* of wine. He went to bed at ten o'clock, after having attempted to eat some soup; but as he was not hungry, he took no more than two spoonfuls.

After midnight, about two o'clock, he was awakened by a violent pain in the right side of the chest, which obliged him to moan, and threw him into extreme agitation. At ten o'clock in the morning M. Leclaire, his physician, found that the pain occupied the whole of the right hypochondriac region and the epigastric. The patient could not bear in these parts the slightest pressure. The pulse was natural. There were neither stools nor vomiting. The urine voided by the patient was of the acajou colour;* the evening before it was natural. The conjunctiva was slightly jaundiced. M. Leclaire, persuaded that it was gall-stone colic, prescribed the potion of Durande.† This the patient took at two o'clock; but some minutes after the first spoonfuls the hepatic pain, which had been slightly abated after mid-day, underwent a remarkable aggravation. The patient screamed, was agitated, could no longer take the potion, declared he was poisoned. The pain lasted in this form till next day, the 29th, when it suddenly ceased at seven o'clock. The patient was able to rise; he had a little appetite and took some soup. The jaundice, which began on the 28th, was increased.

On the 30th, the patient was purged by means of Seidlitz water. He discharged bilious stools but no concretion.

On the 2d of July the patient thought he might eat a cutlet and drink wine diluted with water. But immediately afterwards he was attacked by intense hepatic colic, which lasted for three hours and suddenly ceased.

On the 3d, 4th, 5th, and 6th, the patient took only soup and *potage*, and abstained entirely from red wine. He went on improving, the jaundice declined daily, and he had only a slight tinge of yellow.

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* The *Acajou* is the *Anacardium occidentale*, or Casheir Nut Tree. This seems to refer to the colour of the receptacle, which is crimson.

† A mixture of turpentine and sulphuric ether.

On the 7th, the patient, finding himself well, eat without hesitation a Bret-fish, and drank a glass of undiluted wine. This indulgence was followed by the return of the hepatic colic in its original violence, and lasting longer than ever, for three days. The jaundice also reappeared in all its intensity. M. Leclaire ordered leeches to be applied over the hypochondriac region.

On the 10th, the pain was gone; the jaundice was deep. Soup was allowed.

On the 13th, the jaundice had almost disappeared. Castor oil procured bilious stools but no gall-stone. Soup; then more substantial food. No wine allowed.

The patient continued to improve, and punctually followed the regimen.

On the 15th of August he broke the regimen ordered. He had gone at noon to the barrier with a friend and drank one *litre* of undiluted wine. Immediately afterwards hepatic colic came on. The patient, alarmed, ran into the fields with the view of dissipating the pain, but in this he did not succeed. At two o'clock he came to his physician, who ordered him to bed, and to take a soothing draught. The colic ceased at six o'clock the same day, and was followed by slight jaundice which declined next day. Since this last affair the patient has renounced the use of undiluted wine, and he feels in the right hypochondriac region only trifling and fleeting pains. The liver makes a slight projection in the epigastrium.

The facts now adduced appear to me to demonstrate in a positive manner the actual existence of hepatic colics, produced by the conveyance into the liver of certain articles of food and drink which have acted as irritants to this organ. Though I am unable to explain these results according to the present principles of pathology, they have formed the basis of all the ideas contained in this memoir.

This disorder is not common. It is still less so in hospitals than in civil practice. Nevertheless, if we see not frequently attacks of hepatic colic, properly so called, that is to say, attacks of violent pain, with anguish, breathlessness, and the other symptoms, we observe, frequently enough, persons who, after having taken into the stomach irritating substances, feel a sensation of uneasiness, oppression, and heat in the hypochondriac region.

We must place in the first rank of substances which possess the disagreeable faculty of exciting hepatalgic attacks in individuals predisposed, at first alkoholic drinks taken even in moderate quantity; then green acid fruits; strong acrid condiments, such as mustard, pepper, salt, fruits preserved in vinegar, and similar articles. Lastly, it has been seen that an access of hepatic colic has taken place, a second quite as violent as the first may be, induced by administering immediately after the access of the attack a simple purgative, or the remedy of Durande, or Vichy water. It is requisite during this first moment of extreme irritability of the liver to give the patient emollients in all forms.

In respect to the cause, the greatest analogy, it may be observed, subsists between these pains of the liver and those pains of the stomach which arise after the ingestion of certain substances, for example, fruits, lemonade, condiments. The direct influence of ingested articles upon the production of gastralgic attacks cannot be denied. It is in the same manner necessary to admit that hepatalgic attacks are produced by the same causes acting in the same manner; only we must make allowance for idiosyncrasies, in consequence of which the stomach in one class of persons, and the liver in another class, is the organ which is principally affected upon the contact of ingested articles.

It has been above shown, that the general opinion which makes all instances of hepatic colic depend on obstruction caused by gall-stone scarcely permits us to interpret the two frequent phenomena of hepatic colics; namely, bilious vomitings during the attack, that is, while the *ductus choledochus* is thought to be obstructed, and after the attack the circumstance that no gall-stones are discharged.

It will, however, be quite as difficult for us to understand, by assuming the circumstance of a gall-stone fixed in the biliary passages, those instances of colic which ensue immediately after each introduction of irritating substances, so long as the acute sensibility of the liver continues. What relation of causation shall we be able to find between these ingestions and a gall-stone coming at the same moment and each time, to obstruct the interior of the biliary duct.

It is requisite here for the reader to bear in mind, that when I say that gall-stones do not produce these colics taking place in the manner described after the ingestion of irritant articles, it is necessary to form some understanding of the sort of influence which they may exercise. We must allow, that gall-stones by their mere presence in the gall-bladder or in the substance of the liver, may render the liver irritable, and predispose it to be affected by neuralgia under the influence of irritant *ingesta.* But that it is the gall-stone itself which obstructing the bile-duct produces each accession of hepatic neuralgia; an accession distinguished by bilious vomiting and no discharge of any gall-stone; an accession which takes place after the introduction of aliments into the stomach, and which terminates all at once, when digestion is completed;—this it is impossible to admit.

We are led to distinguish in hepatic colic as in asthma and in gout, the *attack*, which may last for eight days, fifteen days, one month, or which exists, in short, during the whole time that the liver may be affected by pain; and then the *accession* or paroxysm, which is nothing else but the colic properly so called, which lasts from one to two days, and which is repeated more or less frequently so long as the attack is continued, that is to say, so long as the liver is irritable.

It will also be necessary to distinguish the colic from different causes, according to certain characters peculiar to each of them. Thus we may consider as calculous, the colic which is not accompanied by bilious vomitings, which is terminated by the expulsion of a gall-stone, and which is little liable to return by relapse. The rheumatic colic is that which appears under the influence of chilling or a rheumatic metastasis, and which declines on the re-establishment of warmth and transpiration. Lastly, the colic arising from ingested articles will be considered as such, when it is manifested immediately after a meal, when the initial pain proceeds from the epigastrium into the right hypochondrium, when the duration of the colic hardly exceeds that of digestion, and when another attack comes on after the ingestion of any irritating substance. It is well understood, that in these two last species of hepatic colic, bilious vomitings and frequent relapses may take place.

I have every reason to think that hepatic colic owing to *ingesta* is the one which there has been the most frequent occasion to observe. The proof of this I find in respectable authors who have treated of the calculous colic, and who, preoccupied by the established doctrine of the effects of gall-stone, refer without hesitation to the alleged presence of gall-stone, the circumstances symptomatic of the colic determined by the *ingesta*. Of this I shall now furnish proof.

I shall adduce first the authority of the nosologist Sauvages. According to this author the principal characters of *hepatalgia calculosa* are the following. " Hepatalgia calculosa cognoscitur 1*mo*, ex atroci sæpius dolore circa locum ubi canalis choledochus inseritur duodeno ; 2*do*, ex eo quod dolor ille ad costas spurias et epigastrium extendatur ; 3*tii*, quod *tribus sæpius a pastu horis*, recrudescat ; 4*to*, quod auriginem comitetur aut sequatur." *

One of the characters of *colica calculosa*, then, assigned by Sauvages is, that this colic evinces its presence most frequently three hours after the ingestion of food. In what manner is this fact to be understood as to calculous obstruction ? It must be understood that the gall-stone is found in the *ductus choledochus* three hours after the meal; and it must be further understood, that after each meal a new gall stone must be found in the same canal in order to produce each recurrence of the attack.

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* Nosologia Methodica, Tome ii., p. 107.

Soemmering insists upon the same character, and gives it a more exact form, as if to increase the difficulty of conceiving that calculous obstruction can be the uniform and true cause of these attacks. "This pain is wont to come on," says Soemmering, "after dinner; but sometimes, when it arises from the copious use of food alone, it ceases when digestion is completed."*

Here, then, it is supposed that a gall-stone obstructs the biliary ducts after a meal, especially after a copious meal, and ceases to obstruct these same canals after digestion is finished. The question may be proposed, is not all this much more intelligible, by inferring that the liver has become irritable, and is the seat of acute pain so long as it receives alimentary substances more or less irritating and abundant, and which ceases to suffer when digestion is completed?

We find in the Memoir of Durande accurate examples of hepatic colic taking place after meals.†

"The Sieur Bouin," says this author, "machinist to the academy, suffered for six months under hepatic colics, which ensued almost every day two hours after dinner."‡ Subsequently, at page 142, is recorded a case given by Maret, in which it is stated that the patient "experienced for the space of from seven to eight months, colics, the returns of which became more and more frequent. These colics were indicated by symptoms of indigestion." Afterwards, at page 164, he speaks of a patient who, in the month of November, experienced what he calls fits of indigestion. At the third accession of these supposed fits of indigestion, which this time was very violent, I no longer doubted the existence of hepatic colic."

These accessions were not, as the author states, fits of supposed indigestion, but, in truth, a direct attack of indigestion; but the seat of these attacks was the liver in place of the stomach. The liver could not digest but with pain and violent disturbance, the alimentary substances conveyed by the Portal Vein, and conducted itself quite as badly as the stomach, when the latter is unable to endure the contact of alimentary articles without sufferings, cramps, vomiting, and similar symptoms. There are then, as we perceive, attacks of indigestion of gastric origin, and attacks of hepatic origin.

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* "Hic dolor post prandium oboriri solet; interdum vero a solo ciborum copioso usu ortus, facta digestione cessat." S. Th. Soemmering de Concrementis Biliariis Corporis Humani. Trajecti ad Moenum, 1795. 8vo. § 33, p. 57.

† Observations sur l'efficacité du Melange d'Ether Sulphurique et d'Huile de Terebinthine dans les Coliques Hepatiques. Strasbourg, 1790.

‡ Ibid., p. 115.

Authors, however, not only without wishing it, demonstrate the real existence of hepatic colics arising from alimentary *ingesta;* they further tell us of colics similar to those which have been related in the first and eighth cases,—and developed after the ingestion of medicinal substances, for instance purgatives, the mixture of Durande, and similar articles. Thus Pujol, whom I might have cited above, as one who also assigns as the character of hepatic colic, the circumstance of being developed after meals,*—Pujol disapproves of the administration of purgative medicines in the early periods of the disease, because they infallibly arouse the pain.† Of the same opinion is Durande. In all instances, says this author, purgatives usually renew the return of colics, which it is more prudent to avoid, rendering the matter movable, according to the advice of Hippocrates, before carrying it off. Thus, after a long use of the solvent of gall-stones, purgatives may be given to patients without causing pain.‡

This supposed solvent of gall-stones, however, also causes hepatic colic like purgatives, when it is administered in the early stage of the disease. For this reason Durande recommends its employment only after a long use of humectant and diluent remedies. "If this remedy causes agitation, if the region of the liver becomes painful, we draw blood, and continue the use of the baths."§

It is impossible to admit, as has been already remarked, that these different agents produce accessions of hepatalgia, by causing each time a new gall-stone to be fixed in the biliary passages. More probable is it that they act as direct irritants of the liver at a time when it is very irritable. It appears to me that these facts were by themselves of a nature to make the before-mentioned authors return to the system of Galen, by showing them that the ingested articles proceeded to the liver by the channel of the *Vena Portae.*

In modern writings we no longer find examples of these relations of coincidence between the *ingesta* and attacks of hepatic colic. But we may observe in them some facts which tend to a certain degree to invalidate the exclusive theory of obstruction by means of gall-stones.

M. Fauconneau-Dufresne, who has studied with particular care the diseases of the liver, has published a Memoir on Gall-stones, in which the following fact is recorded.||

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* "The cause of colic is almost always found, if it be manifested some hours after an ordinary meal, especially in the case of preceding mental chagrin." Oeuvres de Medecine Pratique, Tome iv. p. 395. Coliques Hepatiques.

† Opus cit p. 408, et seq. ‡ Loco citato, p 90. § Loco citato, p. 9.

|| Memoire sur les Calculs Biliaires. (Revue Medicale, Janvier, Febrier, Mars, 1841.)

"I attended professionally in 1838 General —— for symptoms of violent hepatic colic, the accessions of which were frequently renewed. One of them continued for ten hours, and hrew the patient into a state of despair; when Dr Louis, who was called in consultation, recommended the administration of a vapour bath. Scarcely had the patient been in the bath for five minutes, when he announced with joy that the pain had suddenly passed away."

It is possible, viewing this case strictly, that the disease was in this case dependent on calculous colic, and that the bath had effected the sudden expulsion of a gall-stone. But it appears to me more probable that the *hepatalgia* was of a rheumatic character, and that the induction of a copious perspiration was sufficient to cause the speedy dispersion of this visceral neuralgia.

The same I shall say of those important facts, by means of which M. Bricheteau has been led to recommend the external application of ice in the treatment of hepatic colic. In the Memoir, in which these cases are recorded* we perceive that applications of this kind have dissipated in the course of four or five minutes attacks of the most violent colic. A question here arises, whether these pains depended on the circumstance of the biliary passages being obstructed by a gall-stone, which may have been displaced by the action of the ice; or is it not more probable that the ice may have produced relief by the sedative power which it exerts in the treatment of neuralgic disorders? For upon this point we must not lose sight of the fact, that, besides hepatalgic attacks from *ingesta*, which are frequent, and hepatalgic attacks from gall-stones, which are rare, it is requisite to admit the existence of rheumatic hepatalgia, perhaps even gouty hepatalgia, hysterical hepatalgia, &c., each of which requires a particular method of treatment; and whatever be the cause or the nature of the colic, the *ingesta* of agents slightly irritant, fail not to excite at the very moment an attack, when the disease is in its early stage, and that the liver is then in an extremely irritable condition.

HEPATITIS.

We shall endeavour to show that there are attacks of *Hepatitis* produced by the direct and local influence of ingested articles upon the liver. This article is in a sense the consequence of the preceding one; for if certain substances are capable of producing a fleeting and temporary disorder of the liver, distinguished by violent pain with swelling and determination of blood into the organ, there is no great distance between this and the production of a phlegmasial congestion, fixed and continuous, that is to say, Hepatitis.

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* Clinique de l'Hopital Necker, p. 334. Paris, 1835.

This disorder is seen in its greatest intensity only in hot climates, and particularly in India and the Antilles, where many persons are attacked by it. It is then only by means of documents furnished by physicians who have practised in these different countries that we can establish the pathogenetic point under consideration.

All physicians are agreed in admitting the hurtful influence of alkoholic drinks and irritant aliments upon the formation of hepatitis. Annesley allows that among all the agents which in Bengal tend to induce inflammation of the liver, few possess so much energy as the habit of alkoholic drinks.[*] Another author, who has also practised in Bengal, Dr Twining, informs us, that the immoderate use of wine, alkoholic liquors, and stimulant articles of food, expose the greatest part of Europeans who inhabit India to suppurative inflammations of the liver.[†] According to him we must see in this circumstance and in the atmospheric vicissitudes so sudden in India, the twofold cause of the attacks of hepatic inflammation which are endemial in that climate.[‡]

The same author furnishes valuable information upon the period of the appearance of *hepatitis*, the commencement of which he fixes immediately after taking food.[§] This peculiarity connects hepatitis beginning in this manner with the attacks of hepatic colic produced by certain *ingesta*, and shows, in the collateral appearance of the two diseases of the liver an exciting influence depending on the nature and perhaps the amount of the articles introduced into the stomach.[‖]

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[*] "Among the various influences which more directly occasion the supervention of inflammatory action in the liver there are few more energetic than the immediate addiction to the use of spirtuous liquors."—Annesley, Diseases of India, vol. i., p. 488. London, 1828.

[†] Habitual plethora and superabundance of wine and spirituous liquors, as well as of stimulant food beyond the real wants of the constitution, doubtless keep the greater number of Europeans in India in an almost perpetual state of proclivity to inflammatory and suppurative disease of the liver."—Diseases of Bengal, p. 247. Calcutta, 1835.

[‡] Ibid.

[§] "The attack sometimes commences suddenly after eating."—Ibid. p. 244.

[‖] It may be admitted that if irritating articles, and among others alkohol, produce so easily in hot climates attacks of inflammation of the liver, it is because in these climates the porto-splenic blood possesses little assimilating power, and allows the different agents to penetrate to the liver, without having previously induced in them modification or destruction. It is permitted further to think that this defect of assimilating power partly depends on this circumstance, that in consequence of heat, and the rarefaction of the air, the porto-splenic blood contains an insufficient quantity of oxygen. The contrary of this is seen in cold countries, in which the blood, which ought there to be very highly oxygenated, possesses a great power of assimilation. Persons worthy of credit, who have

ral question arises, in what manner do ingesta act in
hepatitis ?
ow that Broussais imagined that they caused inflam-
rst in the duodenum, then in the common bile-duct
hepatic duct, and, lastly, in the liver. Upon this idea
unded his doctrine of *hepato-duodenalis*, which forms
e hypothetical diseases of the doctrine called physio-
Physiology, nevertheless, hardly permits an hypothesis
or it informs us that a great many irritating substances,
cially alkoholic drinks, to which Broussais ascribed
ly great influence in the production of hepatitis, are
in the stomach, and consequently do not proceed to
num.
authors, who have not been able to admit this opinion
ais, have said that the exciting substances irritated the
and sympathetically caused inflammation in the liver,
uence of the union which connects together the differ-
of the digestive apparatus.
explanation might strictly be regarded as of some
a period when it was thought that absorption was af-
the lymphatic vessels, and the matters absorbed did
hrough the liver. But considering that the conveyance
iver of the matters absorbed by the gastro-intestinal
egarded as an irrefragable physiological fact, we are
it appears to me, to admit, that if irritating substances
ammation in the liver, they produce this effect directly
ntact; and it is not necessary from this result that they
iously caused irritation in the intestinal tube. For as
imes see the large intestine inflamed by means of sub-
hich have come to it, yet without producing any altera-
e superior parts of the digestive tube, in like manner the
undergo an isolated inflammation, from the contact of
hich have traversed the stomach, the intestine, and the

Russia in a cold of 25°, have informed me that they have drank
of distilled spirits without experiencing from them the slightest un-

ument and reasoning now adduced seem to be open to fallacy. No
an be drawn from statements made as to no present inconvenience
rom drinking quantities of distilled spirits in cold countries and cold
any observations show that these articles do not maintain the heat
nly believed, but rather diminish the power of maintaining the heat.
not the question. The point requiring determination is, whether the
tuous liquors does not induce in cold countries many diseases, espe-
of the liver. Now of this there can be no doubt. All testimony and
o to prove that the use of these articles is the principal cause of hy-
irrhosis, and similar affections of the liver. Indeed *kirrhosis* appears
alady of the liver in cold climates, as *hepatitis* is in warm and tro-
ies. In England, Scotland, Holland, and many parts of Germany, it is
isease.)

Portal Vein, yet without inducing inflammation in these different organs.*

Another opinion upon the cause of *hepatitis* in hot climates I ought to mention. As inflammation of the liver is often complicated with dysenteries, which also are endemial in these climates, an explanation of this frequent complication has been given by saying that the purulent matters resulting from the dysenteric inflammation were absorbed by the veins of the large intestine, and conveyed to the liver, in which they thus directly induced inflammation.†

On this point we remark that this explanation can be admissible only to those attacks of *hepatitis* which ensue in the course of attacks of dysentery; and even in this order of cases it leaves something defective. In truth, even excluding dysentery, the liver both in India and hot countries in general must possess a predisposition to be inflamed; for in France, where often prevail epidemics of dysentery, remarkable for the number and severity of the cases, we do not see that hepatic inflammation ever appears as an ordinary occurrence in these epidemics. It must, therefore, be inferred that in hot climates there is some unknown circumstance, which disposes the liver to undergo inflammation, when it is directly irritated either by dysenteric secretions or by certain substances ingested.

KIRRHOSIS.

Although there is nothing fixed upon the true nature of the hepatic alteration to which Laennec applied the name of *Kirrhosis*, physicians are in general agreed as to its most ordinary cause, which is the habit of taking alkoholic drinks. This lesion appears to be more common in England than in France, because the English are more addicted than the French to intemperance and intoxication.

In this instance it is impossible to resist the inference, that since the habit of taking alkoholic drinks, induces in the long run the developement of Kirrhosis, it is because the liver is habitually and directly affected by the alkoholic liquids which are introduced by the Portal Vein.

It may be asked, why, if this be the fact, do alkoholic drinks produced attacks of purulent hepatitis at Calcutta, and only attacks of hepatalgia and kirrhosis at Paris and London? The reason is, that the organism does not react against an identical cause, exactly in the same manner in all latitudes. In Algeria, hillings direct their action principally upon the large intestine, nd induce attacks of dysentery ; in France these exposures to old are most ordinarily productive of attacks of bronchial inammation. In tropical countries punctured wounds of the et and hands are almost certainly followed by locked jaw, *tetanus*); in Europe the same wounds rarely produce this foridable disease. This is the reason why the liver, though subected to the influence of identical exciting causes in India and Europe, is not, nevertheless, in both countries, the seat of orbid manifestations exactly alike.

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* I do not by this deny the existence of attacks of hepatic inflammation produced by extension of inflammation from the gastro-intestinal veins to the liver. Examples of this species of secondary or extended inflammation are given by Ribes. (Revue Medicale, 1825) M. Andral, (Chinique Medical, Tome iv., p. 229, 1831); and M. Bouillaud (Traité des Fiveres.)

† See Note (B) at end of Memoir.

NFLUENCE AND MODE OF ACTION OF THERAPEUTIC AGENTS.

It is readily understood, that, if alimentary substances are onveyed to the liver by means of the Portal Vein, medicines ught also to enter the substance of this organ by the same hannel. As to other points, this is already the conclusion hich we have drawn, as to the subject of purgatives and the emedy of Durande, the administration of which, it has been een, is capable of inducing attacks of hepatalgia.

The waters of Vichy, which exert an action so salutary in he treatment of attacks of hepatic inflammation and hepatic olic, proceed directly to modify the diseased liver, transported s they are to the organic substance of this gland by means of he current of the *Vena Portæ*. I may in like manner remind he reader that, in case first, an attack of colic was produced ome minutes after the patient had taken one glass of the water f Vichy.

Durande, who lived at a period at which physiologists did ot admit the absorption of *ingesta* by the Portal Vein, had his pinion upon the conveyance of this remedy to the gall-bladder, o which it was absolutely requisite that it be conveyed, in order o effect the solution of the gall-stones. " This solvent," says Durande, " is very subtile and very penetrating; it ought in he intestines to be resolved into vapours capable of penetrating nto the biliary duct, and thence into the gall-bladder." This ingular opinion I shall not stop to oppose. I shall merely proose the question if this celebrated remedy really possessed the roperty of dissolving gall-stones. Of this fact I nowhere pereive any positive proof. Durande, indeed, affirms that he had een discharged by the bowels, gall-stones dissolved and conerted into a whitish matter similar to pitch. But what proof there that this matter was a transformation of gall-stones? t cannot be denied that in several instances under the use of his remedy, attacks of hepatic colic had been removed, and hat even in some instances they had been made to disappear so as not to return. But in such circumstances it acted as an antineuralgic upon the liver, and it exerted for the cure of hepatalgic attacks a specific action quite different from that, which has been gratuitously ascribed to it as a solvent of gall-stones.

Admitting that certain evacuants penetrate by means of the Portal Vein to the liver, it would be requisite to explain in what manner these agents clear out at the same time the intestinal canal; but nothing prevents us from adopting this opinion, that the agents before mentioned, having once arrived at the liver, are in it subjected to an elective action, by virtue of which the liver eliminates them from the mass of the blood, and causes them to flow with the bile into the biliary vessels, by which they again enter into the digestive tube. It is very possible that the evacuants, which are supposed to excite an abundant secretion of bile, and which, from this effect, have been justly denominated CHOLAGOGUES by Galen, act in the manner now specified. This question might be easily determined by means of experiments and vivisections.

Not only the alimentary articles, however, even of the most exciting properties; not only medicines arrive at the liver by the channel of the Portal Vein;—the most soluble poisons follow the same route before penetrating into the entire organism and infecting the mass of blood. In proof of this inference, I have only to recal to the reader's remembrance the experiments of M. Orfila upon the subject of arsenical poisoning;—experiments which have in all instances shown the presence of a large proportion of poisonous substances in the liver. The reason of this superabundance it is easy to perceive. The liver being placed partly as a barrier, partly as a sort of filter between the system of the Portal Vein and that of the *Vena Cava*, it is quite natural that the poisons which come to it from the digestive tube, by the current of the Portal Vein, and which must of necessity traverse the organ, should accumulate in its interior in greater quantity than in other organs.

Such are the different pathological facts, which show that matters ingested into the digestive tube are conveyed to the liver by means of the Portal Vein. These facts form as it were the corollary of the numerous experiments, which have been performed in recent times upon the absorbing faculty of the Portal Vein; and the concordance in this respect between physiology and pathology is perfect.

Notwithstanding this, these pathological facts have never been noted, even at the period at which the conveyance of aliments into the liver was admitted as one of the most undoubted principles in physiology. In truth, let any one examine the works of Galen, and of the principal Galenical physicians, and he will not perceive in them any indications of the doctrine, that diseases of the liver are determined by the direct action of certain *ingesta* conveyed into vessels of the liver.

This sort of contradiction between physiology and pathology, which at first sight causes astonishment, is nevertheless understood, when we consider that, according to the ancients, all the absorbable part of the aliments was reduced into one uniform juice, namely chyle, which being prepared so to speak, by nature, as a species of internal milk, never could possess qualities which were to be offensive to the liver. This is the manner in which, as I have already explained in the physiological part of this memoir, the error of considering the chyle as one uniform alimentary residue, has exercised an unfavourable influence upon the pathology of the liver.

We have seen, that in this respect Stahl had proceeded to greater lengths than the Galenical physicians, in so far as he admitted that the *ingesta* designated by him under the names of *acida*, *faeculenta*, and *mucido-crassa*, possessed the hurtful property of producing a degree of thickening in the blood of the Portal Vein. But it appears, that according to Stahl this detrimental influence of the *ingesta* before mentioned, was exhausted in the *Vena Portæ*, and extended not to the hepatic vessels and substance; for we find not in his writings any indication of hepatic diseases produced by this species of cause.

It is only subsequent to the re-establishment of the system of Galen (in a modified form) by M. Magendie, that physiologists and pathologists have published some views upon the pathological and therapeutical consequences of the introduction of *ingested* articles into the liver. M. Magendie first described the direct action of medicines absorbed by the Portal vein upon the liver;* but he says nothing of the diseases of the liver, owing to the conveyance of ingested articles. M. Andral, in considering the influence of alkoholic drinks in *hepatitis*, proposes the question, whether this affection can be produced directly by alkoholic fluids conveyed into the liver?† M. Berard, in the volume of lectures which he published at the close of 1850, admits at once the conveyance of mineral poisons into the liver, in which their presence is proved in great abundance,—and the formation of severe disorders of the liver, under the direct influence of alkoholic liquors, which are absorbed by the Portal Vein.[*] *Lastly*, to these opinions, expressed as pathological consequences of the absorption of *ingesta* by the Portal Vein, it is requisite to add that, by which some authors desiring to account for the presence of *echinococci* in the liver, have admitted that the ovules of these ENTOZOA have penetrated into the alimentary canal along with articles of food and drink, and have thence proceeded by the channel of the Portal Vein into the vessels of the liver, in which they were stopped, and in which they were eventually hatched.[†]

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* "It is not doubtful that medicines absorbed in the stomach and the intestines pass immediately through the liver, and that they must exert upon this organ an influence which appears to me to deserve the attention of physicians." Tome ii., p. 260.

† "Conveyed directly into the liver by the mesaraic veins, is it not possible that the alkoholic particles may determine thus directly upon the organ a powerful irritation."—Clinique Medicale, Tome iv., p. 229.

In order to terminate the pathological division of this memoir, it remains that we speak of the different affections which proceed from any derangement in the peculiar functions of the liver and of the spleen. This second division of the pathological part which I have to treat, ought to be, by reason of its importance, more extended than that relating to the transport of ingested articles viewed as the cause of diseases; but considering the novelty of the physiological facts, which must be employed as the basis of our reasonings, I can do little more than point out the researches to be followed. I shall speak first of the liver, and shall afterwards consider the spleen.

In regard to the liver, the lessons taught by modern physiology show that we have merely to recal to remembrance the principal sanguifying processes of the liver, such as, in recent experiments, they have been demonstrated to take place, and to consider in what particular respects they may be the cause of diseases.

The liver contributes to the function of sanguification, by producing sugar, fat, and by bringing to the state of ordinary fibrine, the soft deliquescent fibrine of the porto-splenic blood. These several functions it is requisite to examine in succession.

The sugar furnished by the liver has led pathologists to think since its discovery on a new theory of diabetes. It has been argued that the sugar, which is formed in the urine depends on the circumstance, that the hepatic sugar is not destroyed in the lung, or on the circumstance that it is produced in excessive quantity by the liver. This latter opinion is the one adopted by M. Bernard.

On this second question, however, physiologists have not yet thought. If in certain instances the liver secretes sugar in excessive quantity, to give rise to diabetes, it may also very well, in other pathological instances, not secrete this substance at all, or produce it in quantity insufficient for the necessities of the organism. What is the disease which results from this defect, or from this insufficiency of sugar secreted? (D.)

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[*] *Loco Citato*, p. 601, 611. Paris, 1850.

[†] See Note (C).

With regard to the fat produced by the liver, M. Bernard professes the opinion, that when this fat is found in the blood in excessive quantity, the result is the state of the name called *chylous*. Let this be granted. But when this secretion is suspended, the question arises, what sort of suffering affects the organism, and under what name has this suffering been hitherto known?

The same question may be proposed on the subject of the function by means of which the liver restores to its state of perfect coagulability the soft fibrine of the porto-splenic blood. If this coagulating faculty of the liver is either diminished or completely suspended, all the blood of the organism will be remarkable for containing a soft and imperfectly coagulable fibrine. And if it be considered that the condition of the blood distinguishes typhous fevers, severe fevers in general, and especially scurvy, the question may be proposed, if, in these different diseases the cause of the said alteration of the blood depends not on an affection of the liver, in consequence of which this organ would allow the portal blood to pass through without rendering it perfectly coagulable.*

Regarding the biliary secretion, it will be sufficient for us to remind readers, that the excess or alteration of the bile has been considered as a powerful cause of diseases, not only at the period at which reigned the system of Galen upon the function of hæmatosis, ascribed to the liver, but even after the fall of this system. It is indeed generally known that the school of Stoll, which acquired great celebrity in the study of bilious disorders, did not look upon the liver as an organ of sanguification.

The pathological study of the biliary secretion, nevertheless, presents a degree of interest beyond this, under the aspect of hepatic hæmatosis; for if, as has been said, the bile is the result of the elimination from the liver of materials improper for sanguification, or in too great quantity, such as carbon, it follows, that when the secretion of bile shall be diminished or suspended, all these materials, however noxious, will penetrate by the suprahepatic veins into the large circulation, and will be capable, by altering the composition of the blood, of becoming a cause of diseases. To this mode of viewing the matter it will be objected, that in jaundice, in which the secretion of bile is suspended, there are no symptoms which indicate the presence of a great disturbance in the organism. This is true. But it may be answered, that in jaundice there always are enfeebling more or less marked of the principal functions; and it is natural to ascribe this debility to an interruption of the biliary secretion, that is to say, to deficient depuration in the function of hepatic hæmatosis. It is further necessary to bear in mind, that all attacks of juandice are not marked by simple symptoms of debility. It is well known that of this disease there are various degrees, and that some attacks of jaundice, though primary, are so severe, that they rapidly cause the death of the patients, without any possibility of preventing the occurrence of the fatal result. It cannot be denied that cases of this class forcibly demonstrate the dangers which may arise from suspension of the biliary secretion.

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* It may doubtless be here objected, that, according to recent analysis, the blood of scorbutic patients is not so poor as it has been said to be, since, on the other hand, it contains more fibrine than in the normal state. I shall merely remark on this subject, that the fibrine may be in excessive proportion, and nevertheless be soft and liquify in the air, as has been shown by M. Beclard, as to the fibrine of the splenic blood. Is it the same with the blood of scorbutic patients? This is a new question for solution.

Lastly, even the subject of febrile heat appears deserving of attention, as a department of hepatic pathogeny. Galen placed the origin or source of animal heat, as is known, in the liver. Notwithstanding the great aversion evinced by Portal to give any attention to this opinion, we are obliged, it appears to me, to assign to it some weight, since the facts established by the last experiments of M. Bernard. This observer has ascertained that during digestion the blood which flows from the liver has a temperature more elevated than that which enters the gland; and that this difference is at least one degree.[*]

We proceed, meanwhile, to the spleen, considered in its pathological relations, and we shall see, if it be possible to demonstrate in the study of certain alterations of this organ corollaries to the physiological exposition formerly given.

We speak, first, of certain agents, such as the alkoholate of quinine and the solution of strychnine, which, when introduced in small quantity into the veins of an animal, possess the property of causing an instantaneous contraction of the spleen, and diminishing its volume.[†] The first agent was employed experimentally by M. Piorry,[‡] and the second was employed in the ame manner by M. Magendie, M. Piedagnél, and M. Bernard. Every one mûst perceive that a like result is obtained, because he agents before mentioned exert their principal action upon he vasculo-areolar element and the proper membrane of the pleen, the contractile power of which is thus over-excited. The spleen being thus reduced in volume and contracted, furnishes a good opportunity for observing the glandular element, he presence of which is brought under observation by the approximation of the granulations of the spleen, the parenchyma of which thus presents, upon section or laceration, the aspect of a gland.

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[*] Union Medicale, 24 Septembre 1850.

[†] A distinction must here be made, to avoid a contradiction between what is here stated and what was formerly said in page 51 of this volume. When the strychnine is given in a large dose, and when it induces general tetanic contractions, it produces an enlargement or dilatation of the spleen, in consequence of the extreme fulness of the Portal System, which is unable to empty itself by the supra-hepatic veins into the *vena cava*. When it is given in a small dose, it acts solely on the spleen, in which it induces contraction.

[‡] Pagés Experiences sur les animaux, vivans, tendant a prouver la diminution instantanée de la rate, sous l'influence de l'alkoholé de quinine. Gazette Medicale, . 972. Paris, 1846.

In certain diseases, for instance, marsh infection, scurvy, typhus, typhoid fever, plague, glanders, &c., the spleen presents an increase in volume with congestion of blood, which is habitually named Enlargement of the Spleen.[*] This enlargement, on the mode of formation of which no precise facts have yet been given, depends on this circumstance, that the blood, which is altered by the marsh poison, by the poison of typhus, by that of typhoid fever, and similar maladies, strikes with atony or with relaxation the contractile tissues of the spleen, that is to say, the vascular element and the investing membrane. The spleen thus affected presents a state quite different from that which we have seen produced under the transitory influence of strychnine and quinine; the vasculo-areolar element, distended by atony, admits a greater quantity of blood than in the normal state, and the whole organ is augmented in volume.[*] Such is the mechanism of splenic engorgement in the diseases before mentioned. Meanwhile, it is requisite to add, that this atonic distension of the vasculo-areolar element may be accompanied with softening or with friability of the tissue of the spleen, which then is torn with the greatest facility.

A little special attention ought to be given to splenic engorgements of marsh origin, because they often at length acquire considerable volume, and then constitute, so to speak, a separate disease.

A natural question which suggests itself is, why the engorgement of the spleen becomes more considerable in cases of marsh poison than in cases of scurvy, typhus, typhoid fever, and similar distempers. The reason is, because the individual affected by marsh poisoning conducts himself quite differently from persons affected by the other forms of disease. He passes very little of his time in bed or in a state absolute repose; he is able, whether well or ill, to attend to his occupations. He is compelled at the same time to march, to ascend ladders, or to work upon ascending planes, whether fasting or after taking food; and this mode of procedure may be continued a very long time. He is consequently placed in those circumstances which we know to be most capable of inducing fulness in the porto-splenic system of vessels. Now, the spleen which has lost in this distemper part of its retractile tonicity, and which is already distended, reacts imperfectly upon these incessant accumulations of blood. The vasculo-areolar element and the investing membrane yield more and more, and at the end of a certain time, variable according to idiosyncrasies, the splenic engorgement may acquire an enormous volume.

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[*] The splenic granulations are in this case separated and dispersed. Further, it is never in these engorged and voluminous spleens that we ought to attempt to study the granulations. If Malpighi has said that the glandular grains of the spleen were more easily discovered in the bodies of animals than that of man, it is because in animals, which are habitually healthy and which die of hemorrhage, the spleen is small, contracted in itself, and so far of a glandular structure. In man, on the other hand, who dies most commonly of disease, and of asthenic disease, the vasculo-areolar element is distended, full of blood, and the granulations are scarcely visible.

We have said that the spleen performs the office of a true heart, destined to impel the porto-splenic blood through the liver in circumstances in which its progress is retarded by some obstacle.* This comparison we may pursue into the domain of pathology, by saying that the chronic engorgement of the spleen, in the instance of miasmatic poisoning, depends on the slow, passive, and mechanical distension of the vasculo-areolar element; a distension and dilatation similar in the mode in which it takes place to the passive and mechanical dilatation of the chambers of the heart.

It may be said of persons having considerable engorgements that they have forced or *thrust out* the spleen. This last expression, which is popular, and which is applied to those who make violent exertions, expresses therefore a physiological idea which is perfectly legitimate; and it is further with the view of preventing this *out*-thrusting of the spleen that runners employ a girdle or belt upon the epigastric region.

After what I have said on the variable volume of the miasmatic engorgement of the spleen, it appears to be proper to distinguish different degrees of this increased volume. The two following may be recognized.

1. In the first degree, the spleen though engorged remains

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* There is always this difference between the heart and the spleen, that the one receives the blood of the veins and propels this blood into the arteries, while the other receives the blood of the splenic artery and propels it by the splenic vein into the portal system. It is true that the portal vein, from its trunk to the hepatic ramifications is, so to speak, transformed into an artery by the adjunction of the Capsule of Glisson. There is still another difference between the heart and the spleen, that in the first of these organs no transformation of blood takes place, while in the second, the arterial blood-vessel is changed into venous blood of a special nature.

concealed under the false ribs of the left side, and does not extrude itself beyond these ribs. The miasmatic engorgement, when it is in this first degree, is not more considerable than that of typhoid fever, typhus, and disorders of the same sort, and it depends, as in these disorders, on simple atonic relaxation of the vasculo areolar element and of the investing membrane. The existence of this may be ascertained during life by percussion alone. It seems to have been unknown to the ancient physicians. For the knowledge of this first degree of engorgement as an incipient and characteristic symptom of marsh poisoning we are indebted to M. Piorry.

2. In the second degree, the marsh or miasmatic engorgement is sufficiently large to extrude itself beyond the ribs, and to project upon the *linea alba* and pubal region. It depends upon this circumstance that the vasculo-areolar element, already struck with laxity and dilated, undergoes a progressive distension by the repeated accumulation of blood which takes place in it during muscular exercise. Its presence is to be estimated, not only by percussion, but by palpation, and even by simple inspection. The older physicians knew and described this extreme degree of marsh enlargement of the spleen; and, as they observed, that it took place only in instances of disease of long standing, they erroneously regarded it as a result of intermittent fever, or of the long continued administration of cinchona. For this reason also they called it *fever-cake.*[*]

It may here be further remarked, that the swelling of the belly, which results from this splenic enlargement, has received several odd names, according to the localities in which the disease is endemial. Thus, in the Department of the Lower Charente, persons having enlarged spleens, are called *Tourteaux*, from the word Tourteau, which in this Department is employed to signify a particular sort of loaf, round and elevated. At Cassaw, in Hungary, the inhabitants give the name of *little pig*, (*petit porceau*) to the splenic swelling, in consequence of the rude resemblance which the swelled belly has to a sucking pig. Further, under this singular title, there was published at Leipzig, in 1746, a dissertation of considerable merit.[†] Habitually, patients affected with splenic tumours are called *Rateleux*.

The second degree of splenic enlargement may be subdivided into two species. In one the tissue of the spleen presents no other alteration than that of extreme dilatation of the vascular areolar element. In the other seat the engorged tissue is at the same time indurated, without, however, being affected by skirrhus. The induration is of a particular sort, peculiar to the spleen, and upon the pathological history of which no accurate information has yet been communicated. In one case observed by M. Nivet, the tissue of the spleen was much more dense than that of the liver;* its colour was nearly the same as that of the muscular structure of the heart; the section resembled that of the liver; but its tissue was not granulated. M. Piorry admits also, that in certain considerable engorgements of the spleen, the splenic tissue is indurated; and in that form of disease the sulphate of quinine uniformly fails.†

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[*] This mode of expression seems to be common to the natives of several countries. It is scarcely necessary to remind the English reader that in the fenny counties of Essex and Lincolnshire, the enlarged spleen was known by the name of *Ague Cake*; and the Germans apply in the same manner the term Fieber-Kuchen.

[†] De Porcello Cassoviensi Adelphi. Lipsiae, 1746.

It is often difficult to distinguish during life whether the splenic enlargement is or is not attended with induration. I must add, that the engorgement of the spleen, whether it be in the first or in the second degree, whether it is indurated or not indurated, is always accompanied with a degree of *hydremia*, more or less considerable, the symptoms of which tend to be confounded with those of the splenic swelling, so as to constitute what ought to be marsh *cachexia.*

I shall here adduce the instance of a man who lately entered the wards under my care at the Hospital Saint-Antoine, labouring under splenic engorgement in the second degree. This patient complained to me of symptoms which are not recorded in Treatises on Pathology, and which are perfectly capable of being conceived, bearing in mind the physiological notions explained in this Memoir.

A man aged thirty-three years., of strong constitution, had gone to the province of Constantine as colonist in December 1848. At the end of eight months' residence, that is, in August 1849, he was for the first time attacked by intermittent fever. Since that period he has never been well; for though the fever gave way at the end of some days, to the administration of sulphate of quinine, the patient always remained pale, void of colour, and feeble, and the intermittent fever often returned at indeterminate periods. In February 1850, six months consequently after the commencement of the first symptoms of marsh poisoning, this patient perceived that whenever he walked a little quickly, ascended a stair, &c., he felt a stitch in the left side, under the left false ribs. After this period this pain continued to increase.

In this state was the patient at his entrance into the hospital on the 30th January 1851.

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* Recherches sur l'Engorgement et Hypertrophie de la Rate. Archives Generales de Medecine. Mars 1838, p. 331.

† Traite de Medecine Iatrique, Tome vi., p. 152.

For a space of twelve days he was affected with accessions of intermittent fever which observed not any very regular type. His complexion was very pale, with abnormal murmurs in the carotid arteries, and a sensation of general lassitude; the appetite was diminished. We ascertained, upon percussion, a remarkable augmentation in the volume of the spleen, which exceeded the ribs by three or four inches. It was easy to appreciate by palpation the projection which the organ formed at this point. Further, this palpation was attended with little pain, and gave not to the hand a great sensation of hardness. The same was the case with the liver, which was enlarged in volume, though to a degree less than the spleen. The liver also projected some centimetres beyond the margin of the false ribs. The most interesting details I regard as the following.

When the patient desires to walk a little quickly, to ascend a stair or acclivity, he feels all at once, in one instant, in the splenic region a pain which forces him to stop or to moderate the rate of his motion. This pain he alleviates greatly by making pressure with the hand upon the left hypochondriac region. When he has taken food the pain is still more severe. He can scarcely walk even with slow steps, and he is obliged to remain in a state nearly motionless during the greater part of the time of digestion, especially if food has been taken in considerable quantity. The patient has in consequence of this requested that he might be allowed to eat often and very little at a time; this request I have granted. He finds himself amended by this mode of regimen. I recommended also to use a girdle or belt from ten to twelve centimetres in breadth, which he is to wear habitually when he is desirous to walk. This is iatended to be an advantageous substitute for the office which the hand performs, in giving a moderate degree of compression to the splenic region, and in thus alleviating the pain.

This patient left the hospital before he was able to use the belt.

But sometime after he went out, there was admitted under my care another man who was labouring under splenic enlargement in the second degree, induced in Africa two years previously. This young man, in like manner, felt a pungent pain in the splenic region when he was walking, especially after meals. I recommended him to apply a firm bandage over the epigastric and the hypochondriac regions; and since that time he has been able to walk, even after taking food, without feeling the pain in the splenic region. Readers must easily perceive that the principal details of the first case now given furnish the clinical confirmation of the statements and inferences made above upon the mechanical functions of the spleen.

We behold at first the engorged spleen becoming the seat of a suddenly advancing pain, when the individual attempts any considerable exercise. This depends on excessive engorgement of the spleen, in consequence of the fulness or large quantity of blood which, we know, is found in the Portal system during locomotion and muscular contractions.

We know also that this fulness is carried to its greatest height when at the same time locomotion and introduction of alimentary articles into the *vena portae* concur. This explains the reason why the patient was obliged, on account of the painful distension of the spleen, to remain in a state nearly motionless during the whole time of digestion, unless he had taken only a small quantity of food. Clinical observation could not afford a stronger or more pointed confirmation of the experiments performed by Mr Dobson on dogs deprived of the spleen, which, it may be remembered, all presented symptoms of considerable plethora, only after taking copious meals.

No notice has been taken in modern authors of this hurtful influence of violent locomotion upon persons affected by splenic disease, especially after taking food; but it is perfectly indicated in the monographical treatises of the old physicians. For instance, Flammerding, among the hygienic precautions which he recommends to individuals labouring under tumours of the spleen, justly enforces the following direction; "exerceat se et ambulet, moderate tamen, ante cibos."* Drelincourt, the son, supporting his opinion upon the strength of respectable authorities, gives the same recommendation to the same class of patients; but this he does in terms very energetic, in so far as he represents immoderate exercise after meals as a thing to be eschewed worse than dog and serpent. "Sed ambulationem vehementem *cane pejus et angue* lienosus declinet statim a pastu, uti prudenter atque perite monent Haly Abbas, Alexander Benedictus, atque Lucas Tozzus."† Lastly, the same precaution is strongly specified in the Dissertation already mentioned, as bearing the singular title *De Porcello Cassoviensi*. Adelphi, the author of this performance, states that the swelled spleen is the seat of a pain which is augmented when the patient attempts muscular exercise, immediately after having taken food. "Is dolor augetur maxime protinus a cibo exercentibus corpus, altiorem ascendentibus locum, currentibus." And he naturally recommends that the patient should repose after taking any meal.

It is requisite, meanwhile, to enforce this wise and rational recommendation, not only in attempting the treatment of these instances in which the spleen is very much enlarged, but particularly in those, in which, when miasmatic poisoning has been of recent application, the spleen is hitherto only in the first degree of atonic dilatation. The observance of this precept will be opposed with efficacy to the progressive increase of this dilatation, and it will be calculated to prevent it from attaining the second degree.

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* De Tumoribus Lienis Leydae 1671, Theses 28.

† Drelincurtii Opuscula. Hagæ Comitum. De Lienosis, p. 782.

But it will be further requisite, with the same prophylactic object in view, to direct that the individual, who is the subject of marsh poisoning, should eat only very little at a time, and that he should at all times wear a girdle or belt, which, by giving a certain degree of support and pressure to the spleen, may prevent that organ from swelling during the time while the portal system is greatly filled with blood.

When we bear in mind, that in the localities in which marsh poisoning is an endemial distemper, there are many individuals with enlarged spleens, who are unable to run, even in a slight degree, especially after taking food, without suffering acute pain in the region of the spleen, and who are consequently compelled immediately to stop, it appears, at first sight, sufficiently natural, that the common people ought to have concluded from this fact, that the spleen impedes the exertion of running, and that persons who have been deprived of this organ must consequently run with great facility. This conclusion is, nevertheless, both erroneous and ludicrous. The inference is, in short, quite the same, as if from the fact that running and great movements were equally constrained and prevented by palpitations and breathlessness, the conclusion were to be deduced, that the heart and lungs are opposed to the free exercise of running and muscular exertions, and that a man could run greatly better if he had neither lungs nor heart.

As has been already said, the spleen, instead of preventing running, facilitates this exercise, because, by contracting upon the blood, by which the spleen is dilated, it contributes to accelerate the movement of the blood, which, being then accumulated in the Portal vein, would take a long time in getting through the capillaries of the liver if it were deprived of the impulse of the splenic contraction. The spleen, therefore, contributes to running. It does so as the lungs and heart; but it suffers also in excessive exertions of running in the sound state, and in slight efforts in the pathological state, precisely as the lung and the heart suffer in analogous circumstances.

From the pathological considerations which have been here submitted, it may be perceived that the condition of persons with enlarged spleens is similar to that of animals which have been deprived of the spleen, in so far as that in both, repose after large meals, becomes a matter of necessity. The reason of this is easily understood. In splenic enlargement, the spleen has lost a large portion of its faculty of contraction, and it is then unable to impel, through the vessels of the liver, the excessive amount of blood and alimentary articles which distend the Portal system after copious repasts. At that time there is in the same system a degree of plethora which is as long in subsiding as in dogs deprived of the spleen.

If, meanwhile, we suppose that the splenic engorgement is complicated with induration, a result in other respects very common, the resemblance is still more close between the man with diseased spleen and the animal that has been deprived of the spleen. In truth, the glandular granules have, in the case of the man, disappeared, and are, as it were, dissolved and lost in the indurated tissue. The spleen, in this case, no longer fulfilling its function of gland, can no longer supply a special venous blood to the assimilation of the Portal Vein. Consequently, in the instance of indurated spleen, insufficiency of mechanical action is at once associated with defect of assimilating action. Yet this represents not the whole amount of the evil. For to this two-fold prevention of the splenic functions, which accompanies induration of the spleen, it is requisite to add the extreme constraint which the presence of a large tumour must cause in the performance of all the abdominal functions. In short, the conclusion is unavoidable, that the organism ought to suffer less from the absence of the spleen than from the engorgement of this organ when it is accompanied with induration.

In concluding, I may advert to an opinion generally adopted among physicians. This is, that there subsists between the liver and the spleen a species of pathological union, and that when one of these organs is altered an alteration in the other is a result almost certain.

Galen first remarked this morbid coincidence. "Hepate læso," says he, "læditur lien."[*] The truth of this aphorism most authors have confirmed, till the time of Portal, who, though the opponent of Galen, delivered in 1813, the following proposition. "It appears that these organs, the liver and the spleen, have mutually such relations, that the one feels sympathetically the affections of the other, but in different modes."[†] If we examine the cases recorded in the works of M. Bailly[‡] and M. Maillot[*] upon Intermittent Fevers, we shall perceive that in almost in all instances augmentation of the volume of the Spleen is coincident with an augmentation in the volume of the Liver.

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[*] Galeni Opera, 2d. 20 H.

[†] Traité des Maladies du Foie, p. 294.

[‡] Traité de Fievres Intermittentes Simples, et Pernicieuses. Paris, 1825.

These morbid associations may be explained by supposing a reciprocal pathogenic influence between the spleen and the liver, and conversely between the liver and the spleen. In the first case, for example, it is conceived that the liver being altered, and its functions being more or less impeded, there results a *stasis* or stagnation of blood in the *Vena Portae* and in the spleen, which at length is affected according to the diathesis and the idiosyncrasy of the individual. In the second case, it may be equally understood, that the spleen, being primarily altered either in its tissue or in its functions, the splenic blood no longer possesses its assimilating faculty, and that the aliments being no longer adequately modified, may proceed to affect the liver, which, according to the different circumstances of climate, of diathesis, and of idiosyncrasy, will present alterations of different species. Let us remember, in support of this second alternative, the important fact given by Fantoni, relating to the female who had undergone extirpation of the spleen. It is said, that, upon inspecting the dead body of this female, the observer ascertained, not only the absence of the spleen, but the fact of considerable increase in the size of the liver. May it not in this case be admitted that the liver, deprived of the important service of the spleen in the process of ante-hepatic assimilation, has been at length irritated and hypertrophied by the transition through its vessels of aliments imperfectly assimilated?

All the questions now stated require to be investigated and considered with precision. It was, nevertheless, necessary to direct to them the attention of physicians, by showing them, that since we have returned in some sense to the Physiology of Galen, it is absolutely necessary that we should place Pathology in harmony with this revived and modified system of physiology. In this consists the progress of science. "Sic Medici in eo viscere quod vitae et nutricatus fundamentum est, diligenter occupari debent."[†]

NOTE (A), p. 271.

It must here be observed, that the author gives not a very correct account of the doctrine of physicians on the cause or causes of dropsy during the eighteenth century and the beginning of the nineteenth. It never was a general doctrine that dropsy arises from the rupture of lymphatic vessels; and though some writers have ascribed dropsy to deficient power in the lymphatics, this was certainly at no time regarded as the sole cause of dropsical diseases. There is on the other hand sufficient evidence in the writings of anatomists, pathologists, and physicians, to show, that dropsy was believed to arise not uniformly from any single cause, but from several; and that even the obstructed state of the veins was not entirely overlooked as a cause of dropsical effusion. The time at which the deficient absorbent power of the lymphatics was admitted among many, was probably about the middle of the eighteenth century. But the verisimilitude of this doctrine was greatly weakened by the frequent difficulty and occasional impossibility that was experienced, in exciting by medical treatment the action of the lymphatic vessels to cure dropsy. In no instance was this more strongly evinced than in the theories of the cause of water in the brain; and the frequent insanability of that disorder, must have been a strong argument against the supposed influence of the lymphatics in producing dropsy.

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[*] Traité des Fievres Intermittentes. Paris, 1836. [†] Riolani Opera.

It would lead to too great digression to give of this doctrine of dropsy such a history as the subject deserves. But some of the notions prevalent on the nature of this disease during the eighteenth century may be understood from the following facts.

In the first place, in so well known a book as the First Lines of William Cullen, the doctrine that obstruction to the circulation in the veins is a frequent cause of dropsy, both general and local, is distinctly stated in paragraphs 1648, 1649, 1650, and 1651 of that work.

In the last mentioned paragraph his words are the following. "Not only those causes interrupting the motion of the venous blood more generally, but further the interruption of it in particular veins, may likewise have the effect of increasing exhalation and producing dropsy. The most remarkable instance of this is, when considerable obstructions of the liver prevent the blood from flowing into it from the *Vena Portarum*, and its numerous branches; and hence these obstructions are a frequent cause of dropsy."

The First Lines of Cullen were published in 1777; and the last edition corrected by the author was in 1784. Consequently, this doctrine must have been known at that time.

2. In the second part of his Experimental Inquiries, published in 1774, William Hewson denies that either "increased secretion, or impeded absorption, or rupture of a lymphatic vessel, gives rise to such morbid collections of water as are found in dropsies;" for the reason that the fluid let out does not resemble that contained within the cavities of the serous membranes in

living animals. He thinks it more probable that there is in these kinds of dropsy something more than an increased secretion or an impeded absorption; that there is a perversion of the secretion, and that the vessels throw out a fluid different from the natural one. This, he says, may happen from two causes; *first*, either from the exhalant arteries being themselves altered by disease so as to change the properties of the fluid which passes through them; or, *secondly*, from the mass of blood being vitiated, or abounding so much with water as to affect this secretion. Hence these dropsies are not primary diseases, but the consequences of others; and a diseased liver, spleen, or lungs, which so often accompany these dropsies, are not so properly to be considered as giving rise to them by causing a rupture of a lymphatic vessel, or obstructing the course of the lymph, as by affecting chylification and sanguification; for when the liver, he continues, for example, is diseased, and the bile deficient in quality or in quantity, the food not being properly assimilated, may make a bad blood, which may affect the vessels, and may let go its water into these cavities."*

Hewson subsequently observes, that dropsies are often the consequence of diseased lymphatic glands, p. 212, in consequence of obstruction in these glands, and that whatever impedes the passage of the lymph into the veins; whether it be thickening of the coats of the jugular or subclavian vein near the termination of the thoracic duct, or a tumour of the aneurysmal, skirrhous, or encysted kind, contiguous to any part of the lymphatic vessels, may prevent the return of the lymph, and may thereby occasion a dropsy or œdema of the parts from which the vessels originated."†

Hewson rather strongly maintains the opinion that dropsies never depend on obstruction of the veins; and that when they seem to do so, it is because the lymphatics, which run along the venous trunks, are compressed, and their interior obstructed.

3. Fourteen and sixteen years afterwards, namely, in 1788 and 1790, William Cruikshank, while he also maintained the influence of the lymphatic vessels, gave, nevertheless, more comprehensive and certainly more just views of the causes of dropsies.

"Dropsies appear to me to be of three kinds. The first is, where there is a general debility in the body. This is felt most in the lower extremities or legs; the extremities of the arteries are so relaxed, that they permit the thinner fluids to escape into the cavities of the cellular membrane, whilst the lymphatic vessels, having lost their tone, do not take up these fluids. Cases of this kind are frequently met with in young people from temporary debility, who have undergone some tedious and dangerous disease, as after fever. From this they frequently recover; but it is a very bad symptom in old age, accompanied with asthma, as it is most probable, that the visible dropsy of the legs is then accompanied with an invisible one, or water in the chest. Women frequently bear this swelling of the legs longer than men, even for years, without any danger. Some men have snstained it for twelve years, without the least decrease of their general health. I have seen the integuments of the ankles hanging over the shoes for months, and yet the patient has perfectly recovered.

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* Experimental Inquiries. By William Hewson. Part Second, containing a Description of the Lymphatic System. London, 1774, 8vo, chapter xxii. p. 194.

† Ibid. p. 215.

"The second species of dropsy is very common, and is that which arises in consequence of previous inflammation of a cavity; and may take place in any habit of body. If an inflammation arise in a cavity, it may terminate in a number of different ways: one of these ways is by an increased secretion of the fluid of surfaces.

"A man receives a blow on the testicle; inflammation takes place, and the consequence is frequently a hydrocele, or dropsy of the tunica vaginalis. A child's brain inflames, and this inflammation ends at last in hydrocephalus, or collection of water in the brain. Pleurisy frequently terminates in hydrothorax, or collection of water in the chest. I have often taken away forty or sixty pints of water, which had accumulated in the cavity of the abdomen, in the few days the peritoneal inflammation had lasted, during the usual species of the child-bed fever. This is to be considered as the substituting a less dangerous disease for another. Peritoneal inflammation kills often in three days, but ascites may last twenty years.

"When the arteries of the part have once got a habit of increasing their secretions, they commonly go on for a long time. The lymphatics may, in some cases, absorb their usual quantity of the fluid of surfaces; but, as the fluid secreted by the arteries often far exceeds the quantity which they absorb, the dropsy is still kept up; or, the lymphatics may be so altered by the inflammation, as not to absorb a sufficient quantity.

"The third species of dropsy is that which arises from some obstruction to the return of the venous blood to the heart. This may happen from the blood of the vena portarum being obstructed in its course to the heart by a skirrhous liver; ascites, or dropsy of the abdomen, may be the consequence. It may happen from inflammatory adhesions of the substance of the lungs retarding the passage of the blood of the pulmonary artery through that viscus. In either case, the venous blood is obstructed in its passage to the heart; the arteries find a greater resistance to their throwing their blood into the veins, and are obliged to relieve themselves by an increased secretion from their exhalants, and thus most probably dropsy is produced in the cavities to which they belong; for when the obstruction is removed, the dropsy disappears.—In a case which I attended some years ago, the patient had dropsical swellings of his legs, at the same time had a hoarseness which had lasted for two years. One morning, in stooping to buckle his shoes, he burst a blood-vessel in his lungs, and from this accident lost about two pounds of blood; both the hoarseness and the swelling of the legs gradually went off, and he continued well for two years after.

" A boy, nine years of age, had for some months dropsica swellings of the lower extremities; at first the pulse was strong and frequent, and the heart gave the aneurismal stroke. I said there was some great disease in the chest. After his death, I was permitted to open the body. The right ventricle of the heart was twice as large, and twice as thick, as the left ventricle; just the reverse of what is natural. The reason of this, I found, was a laminated coagulum of blood, about the size of a small cherry, attached by a peduncle to the beginning of the pulmonary artery, and nearly blocking up its cavity.

" A married gentleman, about 35, became suddenly melancholy, complained amongst other things of being lately gradually unmanned; his legs began to swell, his urine became scanty and turbid, and he could not go up stairs without losing his breath. I suspected water in the chest; in three mouths he died. I opened the body. There was water in the chest. But there was an abscess in the right auricle of the heart, containing about an ounce of pus, and surrounded with coagulated laminated blood to a considerable thickness.

" Dropsies may destroy in a number of different ways.

" The terror of the mind may combine with the debility of the body, in the first species of dropsy, and the patient may be quickly cut off from this cause. In œdema of the limbs, the cuticle frequently ruptures at last, and inflammation of the cutis underneath is commonly the consequence. Whether inflammation takes place from this cause, or whether it takes place in consequence of scarifications, either mortification takes place, which is commonly fatal, sometimes even in three days, or the inflammation, without any mortification, is in this habit of body accompanied with such irritation as to destroy. The hydrocephalus kills by the too great pressure it makes on the substance of the brain. The hydrothorax kills by compressing the substance of the lungs in such a manner as to prevent respiration at last. If the ascites does not kill by increasing the general debility of the body, preventing sleep, digestion of the food, and exercise, and in a great measure impeding the action of the lungs; the operation which is performed to relieve the patient, kills at last, by producing peritoneal inflammation.

The chief circumstance in the diseases we have mentioned is, that the lymphatics do not take up the morbidly accumulated fluid. That this frequently arises from some defect in the action of those vessels, we collect from this circumstance, that medicines now and then stimulate them to absorption, and the morbidly accumulated fluids are removed. At other times, though no medicine whatever has been exhibited, the whole water of an ascites has been removed in three days, from some stimulus in the constitution itself, given to those vessels.*

These views so much more strongly are entitled to attention, because they form as it were the foundation of several of the most correct modern doctrines on the pathological causes of dropsies, and with what is said in the third category, form the best foundation for the distinction occasionally made between idiopathic and symptomatic dropsies. These distinctions show that Cruikshank had observed with much care, and had attentively reflected on the facts which came under his observation.

The second category established by Cruikshank, was soon after in the hands of Rush, Quin, and Parry, converted into an important pathological doctrine, which was subsequently associated in a certain class of cases by Dr Wells and Dr Blackall with the albuminous state of the urine. At a period still later (1827), this pathological condition was shown by Dr Bright to be connected with the granular, and what has since been proved to be the fatty degeneration of the kidney.

The third category of causes of dropsy assigned by Cruikshank is not less important for two reasons. *First*, though Cruikshank expressly devotes himself in this work to the illustration of the Anatomy of the Lymphatic Absorbent Vessels, and the diseases in the production of which they are concerned, yet here he distinctly admits as one of the causes of dropsy, obstruction to the return of the venous blood to the heart. In the *second* place, this cause or category of causes connects itself with a class of morbid changes in various internal organs, both important and of frequent occurrence. To this class belong, in the *first* place, what Cullen names skirrhosity of the liver, what Cruikshank calls skirrhous liver, what Morgagni and some others called tubercular liver, and which is cirrhosis of the modern pathologists; we add hypertrophy of the liver; *secondly*, certain affections of the lungs, as chronic inflammation and hepatization; *third*, certain diseases of the heart; *fourth*, various organic changes affecting the liver and the intestinal tube separately or jointly, as strumous tubercles and enkephaloma; *fifth*, affections of the spleen, as induration, tuberculation, and similar disorders.

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* The Anatomy of the Absorbing Vessels of the Human Body. 2d edition, &c. *By* William Cruikshank. Pp. 115–119. London, 1790.

It seems therefore by no means accordant with fact, to represent physicians and pathologists as universally during the eighteenth century ascribing dropsy to rupture and disease of the lymphatic system. Whatever was the case in France, this was certainly not true of physicians educated in the English schools. Benjamin Rush, it must be remembered, who, from various proofs, symptomatic, pathological, and therapeutic, inferred the inflammatory nature of many dropsical disorders, published his Medical Inquiries and Observations at Philadelphia in 1789. Quin published his Treatise on Dropsy of the Brain in 1790, and undertook to show that digitalis cures dropsies generally, chiefly by its power on the blood-vessels, in abating vascular action.

Lastly, It appears from the posthumous writings of Parry, that the doctrine, that dropsy was in many instances the effect of inflammation, was quite familiar to his mind in the year 1784, in 1790, and 1799, and gradually became during the first ten years of the present century a well-established pathological principle, and the foundation of his practice. This inquiry, however, it is unnecessary to pursue farther, especially as the point has been previously noticed in the twenty-fifth volume of this Journal.*

It would be easy, were it requisite, to adduce evidence proving the same facts from the writings of physicians and pathologists during the beginning of the nineteenth century. But enough has been done to show that the present author has in some manner unknown to us, overlooked the evidence now mentioned.

It appears, consequently, that it is impossible to admit, that "obstruction of the veins, as a cause of the production of dropsy, was either thrown aside during the eighteenth century, or so thoroughly forgotten at the commencement of the nineteenth century, that M. Bouillaud taught physicians that dropsies depended on impermeability of the venous trunks." Independent of what Cruikshank states in this country, John Zacharias Plattner says in his INSTITUTIONES CHIRURGIÆ RATIONALIS, published at various times between 1745 and 1783, " Crebrius autem œdema nascitur, si sanguis non per venas refluere potest."*

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* Collections from the unpublished Medical Writings of the late Caleb Hillier Parry, M.D., F.R.S., vol. i., p. 85, and Edinburgh Medical and Surgical Journal, vol. xxv., p. 366. Edinburgh: 1826.

If, therefore, the fact again made known by Bouillaud were a discovery, it is so by neglecting and overlooking doctrines which were well established,—a source not unfrequent of discoveries.

Note (B), p. 294.

It is not perfectly correct to say, that in the instance of suppuration of the liver, associated with dysentery or ulceration of the intestines, the purulent matter directly excites inflammation. It seems more accordant with the fact to say that the purulent matter is simply conveyed and transported by the veins of the intestines to the *Vena Portae*, and in its ramified branches is there deposited in the substance of the liver. Unquestionably it may produce inflammation. But it is manifest that this is not necessary; simple transport being all that is requisite. The same, it is known, takes place in the *pleura* and in the lungs, from suppuration at the extremities or in the course of veins; and in wounds and injuries of the liver these secondary collections are often seen.

An exception has been taken to this doctrine, that abscess and purulent deposits in the liver do not take place in all cases of dysentery. The objection is perfectly well founded. It is not in dysentery, properly so called, that is, in its early and inflammatory stage, that purulent deposits in the liver are observed. It is only when the disease has proceeded to ulceration, and when ulceration has taken place in the intestinal tissues from other causes, that purulent deposits in the liver take place, or may take place. Let it *first* be remembered, that, to the formation of these purulent deposits a suppurative or an ulcerating surface is requisite; that is, a surface secreting purulent matter, which may be taken up and transported by the veins. *Secondly*, let it be also remembered, that the purulent matter may be secreted, yet may not be absorbed by the veins. Unless, indeed, it is both secreted by the surface and absorbed by the veins opening on that surface, no purulent deposit can be formed.

In the *third* place, let it be remembered, that, in the circumstances stipulated, there is not one but many ulcerating and suppurating surfaces; and it will not be difficult to understand why, in certain cases of dysentery, that is, in the early stage and the inflammatory condition, purulent deposits do not take place in the liver, and why in the ulcerative and suppurating stage, and in consequence of ulceration of the intestines, purulent deposits are formed in the liver. This deposit of purulent matter in the liver we have seen take place in the disease called chronic diarrhœa, simply because that chronic diarrhœa depended on the presence of numerous ulcers in the lower part of the ileum and in the colon.

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* Joannis Zachariæ Plattner, D. et Prof. Med. Lips. Institutiones Chirurgiæ Rationalis, tum Medicae tum Manualis in usus Discentum. Editio altera. Lipsiæ, 1758. 12mo. Editio Novissimis. Lipsiæ, 1783.

Note (C), p. 298.

The whole of the doctrine that alkoholic liquors are absorbed by the veins of the intestines and conveyed directly by the Portal Vein into the Hepatic Vessels, was distinctly and fully stated by Dr Craigie in the second volume of his Practice of Medicine in 1840.*

Note (D), p. 299.

M. Beau makes no progress in the solution of the question as to the pathological deviation which the saccharine reaction in the animal body may undergo or may give rise to; neither does he appear to be aware of all the circumstances of the case. The question is undoubtedly one of difficulty not ordinary. But it cannot tend to solve the difficulty to turn round and ask what are the cases in which the quantity of sugar secreted is below the average?

It may be doubted, notwithstanding the experiments of M. Bernard, whether the liver wholly secretes the sugar found in it. Unquestionably sugar is found in the liver; but it does not thence follow that it is altogether formed within that gland. Dr Prout has shown, that in the stomach there is normally formed a low sugar in the course of the digestive process within that organ. This low sugar is manifestly derived from the *catalysis* and resolution of the amylaceous articles of food, as well as from those which directly contain saccharine matter. Thus, bread of various sorts, rice, the flour of the cereal grains, as well as many vegetable articles, as potatoes, carrots, parsnips, turnips, the tuberosities of the *Helianthus*, called Jerusalem artichoke, green peas, and all similar articles, furnish this low sugar to the juices and action of the stomach. This low sugar must then be absorbed partly by the veins of the stomach, partly by those of the duodenum and jejunum; and probably it undergoes in the duodenum, both by the effusion of bile and the action of the pancreatic juice, a more perfect conversion into sugar. It is at least rendered highly probable that the action of the pancreatic juice upon all amylaceous articles, is to develope and separate more thoroughly the saccharine matter from these articles.

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* Elements of the Practice of Physic. By David Craigie, M. D., &c. Volume Second, p. 73, 74, 75, 77. Brain Fever of Drunkards.

If, therefore, this formation of low sugar begins in the stomach, surely it follows that the liver cannot be the sole and exclusive sugar-making organ in the animal body. Either it must be merely the recipient of sugar for some particular purpose, or it can perform with the stomach and duodenum not more than only a concurrent and auxiliary part in the formation of that substance. In either case the fault cannot be ascribed solely to the liver, that sugar is found in the blood and in the urine.

Let it be remembered, also, that it was clearly proved several years ago by Mr Macgregor of Glasgow, that in diabetes, sugar was found in the stomach, and was formed in that organ.

As to the sugar being destroyed in the lungs, the expression is altogether inapplicable, and communicates no just idea of what takes place in these organs. No doubt oxygen is received, and carbonic acid is discharged by these organs; and it may be admitted to be possible, or even probable, that the sugar is decomposed; that is, that its carbonaceous matter is united with some of the oxygen to form the carbonic acid. But we further know that the system cannot exist without carbonaceous matter; that carbon is found in the muscles, in the bones, in the membranes, in most of the viscera; in short, in all the animal substances.

It seems, therefore, most natural to infer that the saccharine matter which is found in the liver, is merely in that transition state in which it is most easily applied to the nutritious necessities of the system; that its existence as sugar has reference to the primary acts of digestion rather than to any destructive action supposed to take place in the lungs; that as vegetable matters, furnishing sugar, constitute the principal articles of nutrition for animals, so sugar is formed and found in the intermediate stage, though not in the form of sugar in the final stage of nutrition; and that in the final assimilative process in the lungs, the alleged saccharine matter merely undergoes that degree of transformation, which allows the carbonaceous matter to be applied to the repair, the formation, the growth, and the permanence of the animal tissues.

Let it be remembered in these remarks, that pure crystalline sugar does not, and cannot, for a perpetuity, sustain and nourish the human body; that, in the crystalline form, it is positively and unequivocally hurtful and innutritive; that it is always presented to the stomach in the form either of amylaceous matter or general vegetable matter; and that in all animals hitherto examined, the saccharification of these articles begins and proceeds in the stomach. The natural inference from these facts seems to be, that the stomach, *duodenum*, and *jejunum*, require something to act upon; some articles to digest; and that this action, whatever it may be, requires progressive and successive stages. *First*, there is the process of mastication and mixture with saliva, which seems, in the horse at least, and probably in all the ruminant animals, to prepare the way for, and to begin partially, the saccharifying process. Then comes the solvent and catalytic action of the stomach. Next follows the catalytic and separating action of the *duodenum*. Not only must all these processes be requisite, but it is further requisite that articles suitable to undergo these processes be presented as food. Neither sugar nor fat, in their perfect and separate forms, would answer the purpose. Pure albuminous articles, also, are found to be hurtful, and, if long continued, cause various diseases. But we know that amylaceous articles answer, and that from these the alimentary organs prepare sugar, fat, albumen, and gelatine.

It may be, that in diabetes sugar is formed in excessive quantity in the liver. But if the facts and arguments now stated be well founded, then it follows that it is in the stomach and duodenum, that is excessive saccharification takes place. In point of fact, this accords with the facts ascertained by Mr Macgregor.

As to the question proposed by M. Beau, what is the disease which results from the defect or inadequacy of the saccharine secretion? it is surely not a matter of doubt, that one disease at least is known to result from the defective saccharine formation, whether this takes place in the stomach or in the liver. When the system is denied the means of making sugar in the process of digestion, by the want of sacchariferous aliment, scurvy is known to follow as a consequence. Whether it be the only consequence, others are best able to tell. From this fact it seems to be a legitimate conclusion, that, if the stomach were to lose its power of converting amylaceous articles into saccharine matter, even when amylaceous matters were presented to it, either atrophy from inanition must result, or some morbid state of the blood, perhaps an *anæmia*, analogous to scurvy, and equally fatal, would be the consequence. Does the stomach not occasionally lose this power in severe dyspeptic disorders, in which vegetable articles of food are converted, instead of sugar, into acids which act in the meantime as poisons on the system? Is it not to this loss of the catalytic and converting power that is to be ascribed the cause of heartburn, pyrosis, flatulence, and many other distressing feelings familiar to dyspeptics, as well as painful symptoms in remote organs, sometimes assuming a formidable aspect, and imitating or constituting serious diseases.

ART. IV.—*On the Structure of the Membrana Tympani in the Human Ear.* By JOSEPH TOYNBEE, F.R.S., Fellow of the Royal College of Surgeons of England, Aural Surgeon to St Mary's Hospital, and Consulting Surgeon to the St George's and St James's General Dispensary. (From the Philosophical Transactions for 1851, Part I.)

IN conducting researches into the pathological condition of the organ of hearing, I have been impressed with the necessity of ascertaining, with as much precision as possible, the intimate structure of the various tissues entering into its composition. The results to which investigations undertaken with this latter object in view have led me, in reference to the structure of the *membrana tympani*, it is the purpose of the present paper to detail. It will be observed that the conclusions at which I have arrived, differ materially in various important particulars from those of previous investigators.

Examined from the exterior, inwards the *membrana tympani* consists of the following layers :—

1. The epidermis.
2. The proper fibrous layer, composed of—
   a. The lamina of radiating fibres.
   b. The lamina of circular fibres.
3. The mucous membrane.

1. The *epidermis* is a thin layer covering the outer surface of the radiating fibrous lamina; it is continuous with the epidermis of the external meatus, and when subjected to the process of maceration it can be removed in the form of a small blind pouch, which presents, as it were, a cast of the meatus and of the external surface of the *membrana tympani.* When floating in water the pouch assumes the form it had when in contact with other tissues, and its internal extremity is convex, corresponding with the external concavity of the *membrana tympani.* The layer of epidermis forming the outermost coat of the *membrana tympani* is thin, and in the living subject so transparent that the radiating fibrous layer is distinctly seen through its substance: its outer surface is very smooth and capable of reflecting light: this layer presents no appearance of an orifice.

2. The *proper fibrous layer* of the *membrana tympani* can be easily separated into two laminæ, which, on account of the direction of their component fibres, may be called the radiating fibrous and the circular fibrous laminæ. Previous to entering upon an examination of these structures, it is desirable to cite the opinions which eminent anatomists have entertained on the subject.

In the Croonian Lecture, published in the ninetieth volume of the Philosophical Transactions, Sir Everard Home advanced the opinion that the *membrana tympani* in the human subject was muscular. His words are, "When viewed in a microscope magnifying twenty-three times, the muscular fibres are beautifully conspicuous, and appear uniformly the same throughout the whole surface. There being no central tendon as in the diaphragm, the muscular fibres appear only to form the internal layer of the membrane, and are most distinctly seen when viewed on that side."* The use of this radiated muscle, Sir Everard Home states, is "to give those different degrees of tension to the membrane, which empower it to correspond with the variety of external tremors."†

Since the first publication of this opinion as to the muscularity of the *membrana tympani*, anatomists have generally conceded that it is fibrous, but they have widely differed as to its composition. According to Mr Quain and Dr Sharpey, "it is made up of fine closely arranged fibres, the greater number of which radiate from near the centre to the circumference; but within these are circular fibres which are more scattered and indistinct, except close to the margin of the membrane, where they form a dense, almost cartilaginous ring.‡ " Mr Wharton Jones writes, "The proper membrane can be divided into two layers, an outer thin one, consisting of radiating fibres, and an inner thicker layer, which is less distinctly fibrous, though when torn it does indicate a fibrous disposition, and that in a direction opposite to the former.......The fibres which cross the radiating ones are also more aggregated at the centre. They run parallel with the handle of the malleus and turn round its extremity. At the circumference of the proper membrane there is a thick firm ligamentous or cartilaginous ring, which is fixed in the groove of the bone. The ligamentous ring appears to be formed by an aggregation of the circular fibres interwoven with the peripheral extremities of the radiating ones."§

By careful dissection the fibrous layers of the *membrana tympani* may be separated into two distinct laminæ, the fibres of which have no communication with each other. The external layer may be called the *radiate fibrous lamina*, on account of its fibres radiating from the malleus to be attached to the cartilaginous ring, and the internal the *circular fibrous lamina.* The radiate layer is the thicker and stronger. So readily may the two layers be separated from each other, that they are detached with greater facility than that with which the circular layer can be removed from the mucous membrane.

a. *The Radiate Fibrous Lamina.*

If the whole of the *membrana tympani* be carefully removed, there will be observed at its circumference a white dense ring, apparently cartilaginous, which is received into the osseous groove of the temporal bone appropriated to it. It will be remembered, however, that this groove occupies only about five-sixths of the circumference of the inner extremity of the meatus, the upper sixth being smoothed instead of grooved. The cartilaginous ring at the upper part is attached to the malleus, the anterior extremity being inserted into the anterior part of the cervix of this bone, and the posterior extremity into its posterior part; the outer surface of this ring has attached to it the periosteum, lining the external meatus. If the radiate lamina be examined with a magnifying power of ten or twelve diameters, fibres will be observed whose peripheries are attached to the cartilaginous ring, and their other extremities to the malleus. The uppermost of these fibres, however, must be excepted from the observation just made, for instead of passing from the superior part of the cartilaginous ring to the malleus, they take their course in front of the *processus brevis* and form a distinct layer of membrane covering its outer surface. The disposition of this portion of the radiating fibrous lamina is interesting alike to the anatomist and to the surgeon, for it is observed to be continuous with the periosteal lining of the upper part of the external meatus. Mr Shrapnell, perceiving that this portion of the *membrana tympana* was not so tense as the rest, considered it to be a distinct structure, and named it the "*membrana flaccida*." *

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* *Loc. cit.* p. 5. † P. 11.

‡ Elements of Anatomy, fifth edition, 1848, vol. ii., p. 932.

§ Cyclopædia of Anatomy and Physiology, vol. ii., p. 545.

Directly below the *processus brevis* of the malleus the radiating fibres are attached to the ridge occupying the external surface of the bone, but at this part the fibres from each half of the *membrana tympani* are inserted so near to each other that no portion of the malleus is visible when viewed exteriorly; towards the inferior extremity of the long process, however, the fibres are attached to the sides and not to the anterior surface, and thus a small portion of the malleus is visible when viewed exteriorly; towards the inferior extremity of the long process, however, the fibres are attached to the sides and not to the anterior surface, and thus a small portion of the external surface of the long process at its inferior part is left bare and is in contact with the epidermoid layer, as may be distinctly seen in the healthy living ear by means of the *speculum auris* and a magnifying lens. The fibres extending from the malleus and forming the posterior segment of the membrane, are one-fourth longer than those forming the anterior segment. The thickest part of this layer is that which surrounds the extremity of the long process of the malleus, and the most attenuated is that which lies between the posterior margin of the long process of the malleus and the circumference of the *membrana tympani.*[*]

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* Medical Gazette, vol. x.

Structure of the Radiate Lamina.

The fibres composing the radiate lamina, when examined in a fresh state by the microscope, are translucent, and, with the exception of a few transparent granules, present no peculiarity in structure; the longitudinal parallel wavy lines, characteristic of ordinary fibrous membranes, are absent. These fibres are flat and vary from a 40000th to a 50000th part of an inch in breadth. In passing from the circular cartilage to the malleus these fibres interlace with each other, from whence originates the peculiar diamond-shaped markings observable on the outer surface of the membrane. When treated with acetic acid this lamina becomes opake, and sometimes elongated oval nuclei are observed, the long axes of which correspond with the course of the fibres. These nuclei are by no means invariably to be detected, and in several specimens they were not apparent on the application of the acid. In no instance was an oval neucleus visible in an isolated fibre.

The *circular white band* at the circumference of the radiate fibres consists of a firm mass of tissue, which is slightly elastic. It presents an indistinct appearance of fibres intermixed with oval-shaped nuclei. Upon the application of acetic acid this structure loses its white aspect, becomes translucent, and discloses a greater number of the oval nuclei.

b. *The Circular Fibrous Lamina.*

This membrane is attached to the radiating fibres by fine cellular tissue, and, as previously stated, the two structures can be separated with facility; the fibres of each lamina are quite distinct and never intermingle. The circular lamina consists, as its name implies, of circular fibres; they are firm and strong at its circumference, but towards the centre they are so attenuated as to be detected only by considerable care. The strong fibres at the circumference of the layer form a complete circle, and are attached to each side of the body of the malleus and to the sides of the upper third of the *processus longus.* When carefully examined by a magnifying power of thirty to forty diameters, these circular fibres are observed to be intersected by others of an extremely delicate character; these delicate fibres are more numerous towards the central part of the lamina, where they are so intimately blended with the circular fibres that the latter are not easily distinguished. The central thin portion of the circular lamina is not attached to the malleus, but the fibres from each side are continuous, and they form a membranous layer composed of a series of concentric fibrous circles. The outer surface of this central portion of the lamina is in contact with the inner surface of the lower half of the long process of the malleus, to which it is adherent by loose cellular tissue. The circular fibrous lamina is entirely unconnected with the cartilaginous ring into which the radiating fibres are inserted, but it is continuous with the periosteal lining of the tympanic cavity, of which it may be considered a modification. When the lamina of circular fibres is detached from that of the radiating fibres, it will be found to be slightly concave externally, though not to the same extent as the outer layer. In its separate condition it becomes rather less concave than when it was applied to the inner surface of the radiating fibres, and when its central portion is pressed inwards so as to increase its concavity, its inherent elasticity causes it quickly to resume its former shape, resembling that of a saucer. If the two layers be detached from each other and placed side by side, the greater degree of concavity externally of the radiating fibrous layer is very perceptible.

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[*] The blood-vessels ramifying in the upper part of the membranous lining of the external meatus are freely continuous with those of the *membrana tympani.* These blood-vessels are connected by cellular tissue, and the two together often form a distinct layer of membrane covering the outer surface of the upper part of the external radiate fibrous lamina. See Appendix.

Structure of the Circular Lamina.

When highly magnified, the fibres composing the circular lamina are found to be smaller than those constituting the radiate fibrous layer; they vary in size from the 6000th to 10,000th of an inch in breadth. The outer fibres run parallel with each other, aud do not interlace so much as the radiating fibres; they are diaphanous, and do not present any wavy longitudinal lines. When acetic acid is applied the fibres enlarge and assume a certain degree of opacity, and in some instances this lamina also has presented distinct oval nuclei, elongated in the direction of the fibres. Like the radiate lamina, the separate fibres were never observed to reveal the existence of nuclei, and as a general rule their presence in the tissue is not detected.

It is often not easy to decide whether a structure is muscular, and doubts may arise as to the real nature of the two fibrous laminæ of the *membrana tympani.* My own researches do not seem to favour the view of that membrane being a contractile tissue.

The facts which seem to militate against the idea of its being muscular are,—

1st. The absence of distinct nuclei in the fibres.

2nd. Their great denseness and hardness, their firm and unyielding structure, they being so strong that it is with some difficulty they can be torn across.

The *mucous membrane* forming the inner layer of the *membrana tympani* is in the healthy ear so extremely thin that its presence is with difficulty detected; by careful dissection, however, it can nevertheless be removed entire from the inner surface of the circular fibres, to which it is connected with considerable firmness by fine cellular tissue.

It will now be evident that of all the laminæ which constitute the *membrana tympani* not one is proper to the organ, all of them being directly continuous with other structures, of which they appear to be modifications.* Thus:—

1st. The *epidermis* is continuous with that lining the external meatus.

2d. The *radiate fibrous lamina* is a prolongation of the periosteal lining of the external meatus.

3d. The *circular fibrous lumina* is a prolongation of the periosteum of the tympanum.

4th. The layer of *mucous membrane* forms part of the lining of the tympanic cavity.

Previous to considering the functions of the fibrous laminæ of the *membrana tympani*, it is desirable to advert to another point in the structure and relations of this organ. It has been already stated that the *membrana tympani* is attached at its circumference to the temporal bone, and at its central part to the malleus. The latter bone is so suspended by means of the *processus gracilis* and the short process of the incus,† that the long process can move inwards towards the tympanic cavity, and outwards towards the meatus. It must be evident, therefore, that in order to prevent the concave *membrana tympani*, with the above attachments, from remaining in a state of relaxation, either the tensor tympani muscle must be in a state of continual contraction, or some other provision must exist for retaining the *membrana tympani* in the moderately tense condition fitting it to receive the sonorous undulations. The provision which actually exists, and which, so far as I am aware, has hitherto escaped the attention of anatomists, is the *tensor ligament of the membrana tympani.*

The ligament in question is about three-fourths of a line in length, and it is attached internally to the cochleariform process, and externally to that part of the inner surface of the malleus where the long process joins the neck. In the interior of this ligament, which presents a tubular shape, is placed the tendon of the tensor tympani muscle. It is thin anteriorly, where it consists of very delicate fibres, but the remainder is thick and dense, being composed of firm ligamentous tissue. So long as this ligament remains entire and the *membrana tympani* uninjured, the latter structure retains its natural degree of concavity and tenseness; but upon the ligament being cut through, or upon a solution of continuity being effected as the result of disease, the *membrana tympani* becomes very flaccid, even though the tendon of the tensor tympani muscle remains entire. In a preparation, when the tensor tympani muscle is pulled,* the *membrana tympani* is rendered very tense, and the tensor tympani ligament is relaxed; but immediately that the muscle is relaxed the *membrana tympani* is observed to return to its usual state, and the ligament is again rendered tense.

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* See Appendix.

† The short process of the incus is attached to the margin of the mastoid cells by ligamentous fibres, which allow of a lateral motion only.

On the Functions of the Fibrous Laminæ of the Membrana Tympani.

It is obvious that one use of the fibrous laminæ of the *membrana tympani* is to present a firm but delicate membranous septum for the reception of sonorous undulations. The arrangement of the two sets of fibres at right angles to each other has the effect of imparting great strength to the membrane, while it preserves its extreme delicacy and tenuity. It has been stated that there is no evidence to prove that the fibres, of which the *membrana tympani* is composed possess in themselves any contractile power; neither do the component fibres of the laminæ appear to evince more than an extremely slight degree of elasticity. An examination however of the structure after death shows that it has an inherent power of returning to its natural state of tension after being unusually distended. Thus, if the *membrana tympani* be exposed without interfering with its natural state of tension, and the canal containing the tensor tympani muscle be laid open, so that that muscle can be drawn towards its origin, the external concavity of the *membrana tympani* can be increased till it becomes very tense, but as soon as the muscle is let go the *membrana tympani* will be observed to resume its former condition. This action is explainable partly by the slight elasticity of the circular cartilaginous band, into which the peripheral extremities of the radiating fibres are inserted, and partly by the slight elasticity of these fibres themselves, but more especially by the pecular arrangement of the circular fibrous lamina, which it will be remembered has always a tendency, when left to itself, to assume a more shallow form. Thus, when the membrane is rendered very concave, the circular fibres are slightly separated from each other; but when the extra tension ceases, the fibres intersecting the circular ones aid in drawing the latter together again.

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* The tensor tympani muscle has an attachment which appears not to have been hitherto noticed. The tendon is generally described as passing backwards from the muscle, and then turning outwards over the cochleariform process to be attached to the malleus; but previously to changing its direction this tendon is very firmly attached by strong ligamentous tissue to the posterior extremity of the groove of the tensor tympani muscle. When the muscle contracts, these ligamentous fibres are put on the stretch, and the portion of the tendon that passes to the malleus being also stretched, the malleus is drawn in, and the *membrana tympani* rendered tense; but with whatever degree of force the muscle may afterwards be pulled, the *membrana tympani* itself is not rendered more tense in the slightest degree, the whole strain being on the above-described ligamentous fibres attached to the groove. If these fibres are cut through and the muscle be tightly drawn, the *membrana tympani* is ruptured.

The disposition of the central part of the circular lamina also assists it in the function of bringing back the *membrana tympani* to its natural state after tension by the tensor tympani muscle. It has been stated that the middle part of these circular fibres, instead of being attached to the handle of the malleus, are applied against its inner surface, and thus the membrane is rendered tense by the pressure of the long process of the malleus against its outer surface during the action of the tensor tympani muscle, and when this muscle ceases to act the central part of the circular layer of fibres reacts on the malleus, and constrains it to resume its usual position. Besides the office of bringing the *Membrana Tympani* to its natural state after the action of the tensor tympani muscle, the circular fibrous layer would appear to be always acting as an antagonist to the tensor tympani ligament, and by the continued action of these two tissues, the one drawing it inwards, the other outwards, the *Membrana Tympani* is kept in a state adapted to receive all the ordinary sonorous undulations, independent of the exercise of any muscular power.

Appendix to a Paper "On the Structure of the Membrana Tympani in the Human Ear." By Joseph Toynbee, F.R.S., Fellow of the Royal College of Surgeons of England, Aural Surgeon to St Mary's Hospital, and Consulting Surgeon to St Mary's Hospital, and Consulting Surgeon to the St George's and St James's General Dispensary. (From the Philosophical Transactions for 1851, Part I.)

In a note at p. 323 of the above paper, I have stated that "the blood-vessels ramifying in the upper part of the membranous lining of the external meatus are freely continuous with those of the *membrana tympani*. These blood-vessels are connected by cellular tissue, and the two together often form a distinct layer of membrane, covering the outer surface of the upper part of the radiate fibrous lamina." Since writing the above lines I have had opportunities of making several careful dissections of the membrane alluded to, and the result of these dissections is to demonstrate that between the epidermoid and radiating fibrous layers of the *membrana tympani*, there is a distinct and complete lamina of membrane which is continuous with the dermoid layer of the meatus. The presence of this membrane, to which, on account of its structure and functions, I have given the name of *dermoid layer*, is best demonstrated by carefully dissecting under water the membranous meatus from the upper surface of the osseous tube as far as the attachment of the *membrana tympani;* at this point the periosteum of the meatus is seen to become continuous with the radiate fibrous lamina; this being cut through, the dermoid layer is seen passing down over the outer surface of the radiate fibres, and separating them from the epidermis. If the upper portion of this layer be drawn gently downwards by one hand, by means of a fine needle in the other, the delicate cellular tissue connecting it to the fibrous lamina can be broken up and it may be removed entire. The presence of the dermoid lamina may also be shown by the introduction of a fine bristle between it and the radiating fibrous layer, at the superior part of the *membrana tympani*, and then by passing it down the cellular tissue is lacerated.

In a healthy state, when uninjected by blood or by artificial means, the dermoid lamina is thin and transparent; examined by the microscope its structure is found to resemble areolar tissue. When injected, this membrane is observed to have numerous blood-vessels ramifying through it, and they form an elaborate plexus; when these vessels are enlarged, they give the red appearance to the surface of the *membrana tympani* which is so frequently met with during life. It is upon the supply of nerves to this membrane that the exquisite sensibility of the *membrana tympani* depends.

A knowledge of the existence of the membrane here described is of interest to the anatomist, who recognizes in it the secreting organ of the epidermoid layer of the *membrana tympani;* and to the surgeon, who by its presence is able to understand phenomena occurring in certain diseases of the ear which have been hitherto incomprehensible to him. Figure 17 is illustrative of the structure of the *membrana tympani,* and it shows that the six layers of which it is composed are continuous with adjacent tissues.

I may state in conclusion, that the opinion of the non-muscuarity of the fibrous laminæ of the *membrana tympani* advanced n the above paper, has been confirmed by Dr Harrison of Dublin, in his dissection of the *membrana tympani* of the elephant; he says, "I see no evidence of the muscular structure of the *membrana tympani,* so accurately described by Sir Everard Home in the Philosophical Transactions, and mentioned by subsequent writers, who seem to have adopted his opinions rather than to have examined the organ for themselves." [*]

ART. V.—*Upon the True Nature of Hare-Lip and its Complications.* By Dr ADOLPH RICHARD, Prosector to the Faculty of Medicine at Paris. (Archives Generales de Medecine, 4trieme Series, Tome xxv. Avril 1851, p. 419.)

To say that the congenital division of the upper jaw and lip, that is to say, hare-lip with all its varieties, is owing to an arrest of the process of development, is a proposition not at all new in science, and which may, in the eyes of many persons, be viewed as a common fact. But to demonstrate the truth of this fact to surgeons, with anatomical preparations in the hand, and to show that this species of monstrosity corresponds with certain transition states of the human embryo, this is a point of anatomical philosophy which may be regarded as entirely new, and I am in hopes that it will be recognized that the merit of this strict demonstration belongs entirely to M. Coste. •

Bischoff, for instance,[†] who gives a summary of all the labours of his countrymen, particularly those of Meckel, of Reilhert, of Rathke, confines his attention in his chapter on the Development of the Face to two points only; 1*st*, the existence and the signification of the branchial arches; and 2*d*, the ossification of the different pieces of the facial skeleton. He acknowledges himself, as to other matters, that materials are wanting. "Few persons," says he, "have had embryos of mammiferous animals sufficiently young to be able to behold the dependencies of the first visceral arch."[‡] Bischoff, indeed, is able to quote only one figure of Reichert; and in his plates, the only figure which can be found is the profile of the face of an embryo rabbit, showing the branchial arches and nothing more.

Muller, following the steps of Reichert, indicates very well upon the figure of the embryo of a Triton, the general plan of the development of the face. The few lines which he devotes to this subject give much more detailed information than the fifteen pages of Bischoff. But it is merely a sketch, and that sketch traced from a reptile. Not one word of description is given.*

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[*] Anatomical Observations on some Parts of the Elephant. by Robert Harrison, M.D., Professor of Anatomy, Trinity College, Dublin. Proceedings of the Royal Irish Society, vol. iii.

[†] Developpement de l'Homme et des Mammiferes Traduction de Jourdan, p. 395-408.

[‡] Bischoff Entwichelung u. s. w., *i. e.,* Developpement de l'Homme et des Mammiferes. Traduction de Jourdan, p. 399.

Lastly, The magnificent work of Vrolik, though a valuable contribution to Embryology and to Teratology, presents in this department a complete deficiency.†

A question accordingly arises, upon what facts those authors have founded their inferences, who, like Goethe, John Frederick Meckel, Blumenbach, Nicati, Beclard, M. Isidore Geoffroy-Saint-Hilaire, have seen in the hare-lip an arrest of development, and have furnished anatomical proofs of this inference? Goethe, and after him, J. F. Meckel, then Nicati, and recently M. Gratiolet.‡ believed that they found a solution of this difficulty by demonstrating the existence of the incisive bone in man; and it is particularly in reference to hare-lip that the interminable discussion regarding the intermaxillary bone has arisen. But though the presence of this bone is certain in the human fœtus, though its trace is for the most part evident in the adult, though both these circumstances are explained in the same manner as hare-lip, yet this could in no way explain the origin of this deformity. This difficulty Blumenbach, doubtless, perceived; and it is altogether theoretically, he acknowledges himself, that he admits the primary separation of the lip into three portions, which are at a later period united, except in the case of hare-lip. This assertion by Blumenbach, which is entirely gratuitous, has been repeated, upon the authority of his name, by many surgeons and even anatomists, and further, Blandin, in order to accommodate the theory to the instance of median hare-lip cited by Nicati, has not hesitated to admit four points of formation, in place of the three recognised by Blumenbach.

It cannot accordingly be matter of astonishment that, resting upon such proofs, the theory of arrest of development should be and still is rejected by the most respectable scientific men. It is perfectly reasonable that the most intelligent persons should agree in declining to adopt a theory, for the reason that observation cannot demonstrate its truth.

Amidst several other authorities, I shall here quote only Velpeau and Cruveilhier.

In 1833 appeared the Work of M. Velpeau, and it excited among physicians the taste for embryological studies, by showing them the high importance of these investigations. It is scarcely necessary to search in this performance for details upon the development of the jaw, as the first part of the work was devoted only to the history of the appendages of the fœtus. However this may be, after reminding the reader that certain authors were desirous to explain the occurrence of hare-lip by means of the arrest of development, M. Velpeau expresses himself in the following terms. "I think I am entitled to affirm that such ideas have arisen only from inattentive observation. Upon embryos of six weeks, as upon others which were not more than fifteen or twenty days from conception, I have found the margin of the two lips perfectly formed and without any division."*

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* Handbuch, u. s. w. Manuel de Physiologie, T. ii., p. 723.

† Vrolik Tabulæ ad illustrandam embryogenesin Hominis et Mammaliam, tam naturalem quam abnormem. Trajecti. 1849.

‡ Annales Francaises et Etrangeres d'Anatomie et de Physiologie, t. iii.

Observation, nevertheless, could not fail to give him a glimpse of the truth. He adds, "When the contrary took place, and of this I have had numerous examples, it was easy to see that an alteration either pathological, or purely mechanical, had been the cause of this. This is seen in the figure given.

The opinion of Cruveilhier is given in his most recent work in the following terms.†

"Hare-lip seems to be the refuge of the theory of the law of Arrest of Development; and notwithstanding, in this point still, this theory is far from being impregnable. The question may be proposed, is there a period of the embryonic and fœtal life in which the product of conception presents in the upper-lip two divisions, as seen in double hare-lip; one single division, as appears in single hare-lip. Is the lip developed by two lateral halves, which, though at first separate, are to be united upon the median line, or by three points, as is represented by Blumenbach, or by four points, as is the opinion of those who admit the same number of points of formation for the lip, as for the superior maxillary bone?"

"Is it indeed the fact, that there are two intermaxillary bones in the human fœtus as in the bodies of the lower animals, and that the incisive suture of the superior maxillary bone in man is the indication of two distinct points of ossification during the period of intra-uterine life? These questions appear to me to be not yet determined, and I am therefore obliged to suspend my judgment."

The answer to these questions may nevertheless be read correctly in the Anatomical Preparations of the College of France. I shall attempt from these to render intelligible the principal phases of the developement of the face in the human embryo.

I take it at the commencement of the third week, and with the view now stated, I follow the figure given by M. Coste, from the designs of his prosector, M. Gerbe,—a work without equal in science.

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* Embryologie Humaine.

† Traité d'Anatomie Pathologique Generale, tom. i., p. 205, et suiv. Paris, 1849.

At the period specified, the embryo, five millimetres in length, still shows the blasto-dermic aspect. From the outline of the abdomen, which is widely open, proceeds behind the amnios, anteriorly the *vesicula umbilicalis*, without stalk. The intestine from the stomach to the rectum is only a groove; and all that is formed into a tube, is only the œsophagus and the end of the large intestine, from which arises the allantois. In the superior closed part of the embryo, which may be named, if it seem proper, the cephalic pouch, and which is subsequently to constitute the head, the neck, and the chest, the three membranes of the blasto-derma are distinctly separated and already individualized; but in this respect there exists a contrast between the upper and the lower part of this pouch.

The bottom of the cephalic pouch indeed through a serous transparent membrane, without perceptible organization, which is afterwards to be the thoracic case, exhibits the heart still single, but convoluted, (a vascular membrane), and farther back the œsophagus entirely formed, (a mucous membrane). At the top of the cephalic pouch, on the contrary, the serous membrane becoming thick in certain places, becoming thin in other points, or entirely disappearing, conceals the superior *ampulla* or chamber of the œsophagus, which is subsequently to become the pharynx, and thus traces the first outlines of the human face. At this time, indeed, an appendix of the first cerebral cellule, or, if you will, of the frontal region, triangular, with the apex truncated below, proceeding downwards, and on the other side two lateral sprouts proceeding from the middle cerebral cells, and placed over the first branchial arch, converging towards each other towards the apex of the frontal triangle, a sort of mouth is thus traced; for at the level of this opening, the serous fold or membrane possesses the tissueless transparency of the thoracic case, for instance, while the blastema deposited in the frontal and lateral sprouts, allow these outlines to be seen. This central portion or primitive mouth, which is not opened, is then bounded above by the obtuse apex of the frontal sprout, upon the sides by the free extremities of the lateral sprouts, (the subsequent jaws) and below by the first branchial arch.

When a process, which doubtless already commenced, proceeds to excavate behind this last organized portion of the external membrane of the blastoderma a cavity (the pharyngeal cavity), to attenuate and destroy its anterior part, in order to constitute an open orifice, this orifice becomes a sort of *cloaca*, common to the buccal and nasal cavities; and it is exactly to the point of separation of these two cavities that the ulterior development particularly is directed.

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* Plate second of this large work of M. Coste; Histoire Generale et Particuliere du Developpement des Corps Organisés, Tome I. 2d Fascicule, 1849

Now, at this moment, the furrow which separates the external margin of the frontal sprout or process from the superior margin of the maxillary sprouts, ends externally, and in some sense, behind, in the spot at which the eye is very soon to be placed; while within, rising and swelling a little, the aperture of the *nostril* is in no long time formed. This fissure in other respects is capable already of receiving a remarkable interpretation; it is the first indication of the canal for the tears, the lacrymal duct, and nasal canal. I have said that anteriorly, this fissure is swelled and elevated; in this manner the primitive frontal sprout is found to be separated into three parts; a median without fellow, (the *incisive sprout*); a lateral, on each side, (the sprout of the *ala nasi.*) This arrangement may be recognized at the thirtieth day.* The eye begins to be perceived alltogther upon the sides. The incisive sprout enlarged has descended a little below the nostrils. Already, on the sixteenth day, the internal sprouts showed each a slight fissure, which is the separation of the superior and inferior mandible. Meanwhile the two jaws are altogether isolated; but their developement is in a state of singular antagonism. The two halves of the lower jaw are already united in the median line, and the lower circumference of the mouth is henceforward invisible. On the other hand, the jaws above, remarkably distinct from each other, are united only upon the sides of the head, and leave between them and below the incisive sprout, the opening of the cheeks largely divided.

The rest of the developement of the face may be expressed in one word. Gradual approximation of the process of the upper jaw, adding, however, that, besides their external part, each of them presents at its internal surface an antero-posterior and transverse prominence, which at first is scarcely visible, but then extends successively towards the median line. These are the two palatine processes. At first they leave between them a large fissure, by which the nasal and buccal cavities communicate extensively. This fissure diminishes daily and hourly by their continued progression, until, meeting and uniting they constitute the entire palatine vault. All this is accomplished at the same time, and as it were in a parallel manner. Jaws, properly so called, and the half palatine arches, advance in concert towards the median line. Already, as to the adult, these parts form one single and selfsame system; so well, that when the palatine arch is formed, the buccal orifice is in like manner completed; the buccal and nasal cavities cease to communicate externally at the same as internally.

Such is in a short summary the general mode of the developement of the face.

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* See Plates 2, 3, 4, in the Works of M. Coste, and the Explanations.

1. Above the lower jaw, closed at an early period, and bounding as it were below the field of the embryogenic process.

2. The advancement towards the buccal centre of the three appendages of the cerebral cellules; one median (naso-incisive) proceeding from above; two lateral (the superior maxillary bones).

3. The nose forming itself in the unmatched appendage, or that on the median line.

4. The mouth gradually circumscribing itself by the meeting of the three appendages.

5. Lastly, a formation more deeply seated, and synergically giving rise to the palatine arch, as if a dependence of the lateral appendages, to the *septum narium*, a manifest offshoot from the naso-incisive appendage.

There is no necessity for lengthened commentaries in order to show how the explanation of the true nature of hare-lip, and of all its varieties naturally flows from this succinct description. To take a striking example, any one may perceive in the cases altogether extreme, in that deformity which is popularly named Wolf-Throat, the persistence in its entire extent of one of transition states of the embryo.* In this, as on the thirtieth day of fœtal existence, the observer beholds the nasal and buccal cavities forming only one large common funnel, the separation of which is scantily indicated by the *palatine processes* detached from the internal surface of the superior maxillary bones. Then internally between the separated halves of the jaws and of the lips, he observes the isolated naso-incisive eminence, (soft parts and hard parts), strongly projected forwards, and supporting behind the *septum* of the nose, which remains suspended and free in all the other directions. He may be struck, in this instance, as in the fœtus, by this nasal dependence of the upper lip and upper jaw, by this slow and distant complement, which advances from above to join itself to the jaw.

What has now been said is applicable to the extreme cases.

Imagine next all the possible degrees of transition to the most simple form of hare-lip; for under this form, which is traced in the face of the embryo, we must not attempt to consider the skeleton apart from the soft parts. This mode of separating the bones from the soft parts in these reasonings has probably been productive of errors, particularly in reference to the *osteo nervous axis.* In this member, thoracic or abdominal, which at the fortieth day, for instance, may be perceived pointing under the form of a shoot or sprout, all the elements, cutaneous, muscular, bony, vascular, were virtually included; and the proof is, that, if its evolution is stopped, all these different elements are observed in it at birth. In like manner, the incisive shoot or sprout, consisting at first of one almost single *blastema*—of cellules nearly similar each other ; this incisive sprout is very soon individualised into in, muscles, bones, vascular and nervous elements; and this, ether it is to be united to the mandibles, as in the immense jority of cases, or is to remain more or less detached, as in the ceptional circumstances, which are at present under consi- ration.

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* See for instance the series of anatomical preparations in the Collection of the Dupuytren Museum.

Before this synergic development of the lips, of the jaw, and of e palate, it might cause to the observer astonishment to see this est of developement strike almost in all instances only the per lip. But the question may be proposed, why the lip itself not sometimes divided, except in a small part of its extent? hy should the *uvula* be divided, and the palatine arch be left tire, and reciprocally? All the suppositions which the view of e development of the face may suggest, are found realized by merous examples. The anatomist, doubtless, is unable to ex- ain all these varieties, and equally unable to assign the reason y hare-lip is more common on the left than on the right side; r is he more able to say why the arrest of development takes ce at any one given point. All that he is capable of affirming that each of these deformities is, as it were, capable of being perposed upon a transition state of the embryo.

T. V.—*On the Pathological Anatomy of Cretinism.* By Dr C. Lasegue. [Archives Generales de Medecine, 4trieme Serie, Tome xxvi.] Paris, August 1851. P. 385.

It is a common fact in the history of diseases, that the re- rches to which they give rise, change their form according to time at which they originate. The subject remains the same, t the attention of inquirers is fixed upon other points. A new angement of facts and questions is established; the point from ich the observer contemplates the disease is entirely changed. Though this law appears to be almost general, it appears that etinism escapes its power. Since the appearance of the earliest nographies, neither the position of the problems, nor their so- ion, has in a sensible degree varied. Inquirers were struck at beginning by the singular spectacle of a degenerated popula- n, collected in certain insalubrious villages of the Alps. The nptoms were in a manifest manner presented to travellers; the scriptions were easily made, and were faithfully given; the patho- gical conditions were stated in sufficiently general terms to justify cribing only a secondary importance to the perfect examination individual cases. In consequence of this unity of view, a tain number of facts are, at the present time, the property of ence, and are placed beyond the boundaries of controversy.

The complete memoirs of Rösch, of Guggenbühl, of Maffei, and a few more, have annexed learned details to the researches of Wenzel, of Fodéré, and others of the early writers. The Report of the Commission of Sardinia gave confirmation to opinions which wanted nothing but the authenticity of an administrative Inquiry.* Lastly, the [illegible] summary given by M. Ferrus has contributed to arrange, in a valuable analytical sketch, all those facts which must henceforward be regarded, as on this subject, incontrovertible.

The desiderata, or defects, nevertheless, specified by the early observers on Cretinism, have, for the most part, remained in a stationary condition. Rösch, with that sagacity by which he was distinguished, [illegible] physicians [illegible] most satisfied with studies of a general subject. He [illegible] them to investigate the [illegible] causes of [illegible], and showed the necessity [illegible] and [illegible] task, for accomplishing [illegible] villages [illegible] reside [illegible] survey of [illegible]. To [illegible]. At present [illegible] are few. It [illegible] than to [illegible] silence [illegible]

[illegible]

them to aid his inquiries by all the means in their power; of this letter, the substance deserves to be made known here.*

The Surgeon Malacarne, he writes, has obtained all the results that have been possible for him from the three imbecile persons whose bodies were placed at his disposal. He has made of these an anatomical examination. More particularly, he examined the heads of these persons externally and internally, and has uniformly observed the following phenomena. 1st, the skull is generally less elevated and less flattened upon the sides; 2d, the *foramina* of Valsalva greatly enlarged; and 3d, conversely, the *Foramina lacera* of the base of the cranium and the petrous portions of the temporal bones almost obliterated, so that scarcely can the pairs of nerves pass through these *foramina*; 4th, the lateral sinuses are greatly enlarged in all their dimensions; 5th, the *Tentorium cerebelli* is greatly too thick; 6th, consequently the cerebellum itself is lodged in a cavity much smaller than it ought to be; 7th, he observed also in the three imbeciles, that the basilar process of the occipital bone, instead of advancing forwards with a gentle obliquity above, forms a plane almost on a level with the clinoid processes of the sphenoid bone, and the *foramen magnum* opens not as usual from above downwards, but from behind forwards; 8th, the effect of this is, that the mass of the *cerebellum* remains smaller in proportion to the impediment given to its developement; 9th, thus the number of the laminar leaflets and of the plates of the *cerebellum* is not equal, as in other men; 10th, astly, that the *medulla oblongata* is necessarily bent, in reaching he spinal canal, in a manner very hurtful to the nerves which issue from it.

Limiting thus his observations, Malacarne perceived the defect of explanation. It is impossible to admit that a disease, which, ike Cretinism, impresses upon the whole economy a stamp so characteristic, can be an affection purely cerebral. The objection might be stated, that there exists between Cretins and Idiots deprived of intelligence to the same degree, remarkable differenes. He further attempts to account for other lesions; he asks f goitre might not be the cause of the determination of blood, f it might not contribute to the bad direction of the cuneiform process and of the occipital *foramen.* The fact already well ascertained of Cretins without Goitre is sufficient to subvert this inference, though otherwise modestly deduced.

Malacarne had obtained a glimpse, if not of the anatomical truth, at least of the direction in which it was most expedient to conduct subsequent investigations. Fodéré was much below the surgeon whom he undertook to criticise. Upon the ground of one single anatomical inspection, he attempted to describe lesions peculiar to Cretinism and to construct an appearance of system. The observations of Morgagni upon the state of the brains of the insane, supplied to him better arguments. After having recapitulated the facts borrowed from this author, he adds, that having found the brain in two perfect Cretins, smaller, harder, and more dense than usual, he concludes that it is possible, that these changes may be the cause of Cretinism. He cannot attribute Cretinism to excess of stiffness, for the state of contraction of the bones of the skull, the hard olive coloured skin of the Cretins, their [illegible] short hair are probable accidents of the state of con[illegible] hardness, and dryness of the cerebral substance.*

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* Vincenzio Malacarne Lettre sur l'Etat des Cretins. Turin, 1788. Extat in Frank Delectûs. Tomo vi. to p. 241.

Thus Fodéré not only published with a certain degree of pomp, a fact of which the value as the observation was superficial; but he entered on an erroneous path. The bones of the skull were not even [illegible]. The state of the organs was not [illegible] and [illegible] condemned by anticipation every [illegible] of research [illegible] that he never believed that [illegible], [illegible] of understanding, were a necessary [illegible] deformation of the brain or of [illegible] in the state of the organ itself or [illegible].

[illegible] materials for his [illegible] only the three [illegible] previously described [illegible] for a long [illegible]. He recapitulated [illegible] directing [illegible] of the base of the cranium, [illegible] of the cerebral [illegible] upon the cir-[illegible]

[illegible] were [illegible] the [illegible] the mode of [illegible] was unable to satisfy [illegible] causes of [illegible] the study [illegible] of the osse-[illegible] remarked. [illegible] developed [illegible] When [illegible] makes [illegible] in the

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[illegible]

bones perform a part so important, their evolution ought to correspond to the phases through which the disease passes. One lesion alone among all the alterations to which the osseous system is subjected fulfilled these conditions, and fulfilled all of them. This was Rickets. Once possessed of this fertile idea, Ackermann followed it in its most extensive developements. With the Rachitic conformation were associated the swelling of the articulations and the articular ends of the bones, the unsteady gait, the heavy stupid physiognomy, the faulty conformation of the chest, the deviation of the vertebral column from its normal direction, which carries the head forward, the thickening of the jaws and the prominence of the orbits. Meanwhile the Rickets of large towns,—that which in his time prevailed to so great an extent in Holland, exerts no influence upon intelligence and affords no aid to explain the origin of Cretinism. The members are deformed in a different mode; the head has not the same aspect as the Cretin skull. To this objection, which he foresaw, Ackermann made answer, that the disease had not attained so intense a degree as in the case of Cretinism. The explanation was without substantial value; but at the present time we may be pardoned for saying, that we understand more correctly what it was his intention to say. Ackermann is the first pathologist who got a glimpse of the local position of rickets. He recurs constantly to the morbid changes in the skull, to the different deformities which the bones undergo, to their attenuation in some spots, and their thickening in others, to the secondary osseous productions, to the lesions of the spongy bones: he explains the transitions through which softening of the bones passes before it proceeds to eburnation or ivory-like induration. If Ackermann had ventured to say, that the rachitic disease of the skull may follow an independent progress, and that the softening of the bones of the members does not afford the measure of its amount, he would have invested his idea with a substantial existence.

The treatise of Ackermann has seldom been estimated at its just value. Opposed by refutations from physicians ignorant of the question, or who confound Rickets with Scrofula, it has been ranked among the list of those hypotheses which are quoted in order to show erudition. Whether it be right or wrong in relation to the formation of Cretinism, to it we are indebted for one of the most correct performances upon the pathological anatomy of rickets.

By the publication of their work in 1802 at Vienna,* the brothers Wenzel established the transition between the physicians who have now been mentioned and the modern observers. Their treatise upon Cretinism is accurate, strictly methodical. All that is here to be noticed relates to what they have done for the advancement of the anatomical points of the question.

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* Joseph and Karl Wenzel, Ueber den Cretinismus. Grosse 8vo, seite xxiv., 246. Wien, 1802.

Uninfluenced by all preconceived opinions, little solicitous to create a system, these authors have rendered themselves eminent by fidelity of their description, against which no censure can be pronounced, except the excess of sub-divisions and a certain degree of dryness. All the organs are examined successively and in separate chapters, as are all the organic apparatus and all the functions. In absence of a formular doctrine, the authors are nevertheless obedient to an idea which was predominant in their times. All the writers, of whose researches accounts have been already given, take their departure from a sort of principle, or rather from a postulate, which they think not even of discussing. In their eyes, Cretinism is a Unity, which may indeed vary in degree, but which cannot change its form; it is a strict type to which identical lesions ought to be referred. To study the Pathological Anatomy of the Cretins is to investigate that uniform alteration which stamps upon all the patients exactly the same character. This principle the brothers Wenzel explicitly adopt, without following the method in a servile manner. They establish more facts and observe them better, but with the assumption that one anatomical inspection gives a sufficiently good summary of all the anatomical inspections.

It is impossible, say the Wenzels, in the present state of our information, to describe the characteristic shape of the skull of the Cretins; but it is more than probable, that we shall be able to determine this shape as exactly as that of the skull of the negroes. At the present time even we are entitled to abide by suppositions which rest on good foundations. The cranial vault is enlarged, the sutures are persistent, or are separated by intermediate bones of different dimensions; the frontal portion of the skull has little elevation; at the base of the cranium the bones appear to be much crushed together, and consequently they assume a disposition quite contrary to the excessive expansion of the superior portion; the occipital *foramen* is deformed and unshapely, its margins are swelled and rugous. It would be possible to give a sufficiently good summary of the whole of the deformities by supposing that the sterno-cleido-mastoid muscles have exerted a violent and more or less irregular force of traction upon a cranium previously softened. The chest is a little flattened upon its lateral regions.

The authors know upon the state of brain only what they had learned from Autenreith, from which they borrow an interesting anatomical inspection. The left internal ventricle was distended by a large quantity of fluid; its *parietes* were in some sense cartilaginous; the right ventricle had undergone no alteration.

The cranium appeared as if it had been turned round and carried back on the left side.

Subsequent to the period at which Joseph and Charles Wenzel published their treatise, the attention of physicians was for a long time either turned away from or little directed to the subject of Cretinism. In 1817, Augustus Ernest Iphofen published at Dresden a Philosophical and Medical Essay, containing not a few interesting observations on the Nature and Supposed Causes of the Distemper. But, with the exception of this treatise, few memoirs of importance, and only occasional cases were published during the first twenty-five or thirty years of the nineteenth century. Iphofen, further, beyond the well-known anatomical inspection of the Cretin, Karl Niedner, occupies himself particularly with the examination of the skull. He confirms the correctness of most of the deformities recognised by his predecessors, he insists upon the porousness of the bones at the base of the skull, and particularly upon the defect of symmetry, which he converts into a character of Cretinism, and to which he is the first person who ascribes a legitimate importance. Joseph and Charles Wenzel had already remarked this anomaly which they regarded as peculiar to Cretins.

Iphofen has further the merit of having pointed out a mode of investigation, previously neglected, in ascertaining by comparative measurements, the dimensions of the different cavities into which the entire cranium is resolved.

In 1831, Henry Demme published at Wurzburg an Inaugural Anatomico-Pathological Dissertation upon Inequality in the Size of the two Cerebral Hemispheres; and in this performance he took occasion to adduce new examples of Cretin heads, and further to show that the brain may be void of symmetry even in instances in which the two halves of the cranium appear externally identical.*

We must pass over in silence some of the most remarkable researches to which the subject of Cretinism has gave birth. Authors, whose attention was strongly attracted by other questions, rather neglected the question of the pathological anatomy of the Distemper. Statistical returns, valuable geographical documents, and able attempts to treat the disease and amend the condition of the sufferers, learned and faithful pathological descriptions in different degrees were the qualities which recommend several of the works which have appeared since the year 1843. The treatises of Rösch and Maffei which have been elucidated by the annotations of Guggenbühl, hold the first rank.* If the authors have added few facts to those which previous anatomical inspections of bodies had communicated, they have nevertheless given to the anatomical study of the distemper a new direction. In order to show and enable readers to form an estimate of this scientific movement, which is in a great degree owing to the initiative now mentioned, it is sufficient to advert to the inquiries and the writings of Stahl.

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* August Ernest Iphofen der Cretinismus, philosophisch und Med. Untersucht, 2 Theile. Mit. 7 Kupfertafeln, grosse. 8vo, Dresden, 1817.

Charles Frederick Stahl has published two memoirs on the subject of Cretinism. The first is inserted in the NOVA ACTA ACADEMIAE CURIOSORUM NATURÆ for 1845.† The second was published in 1848, and procured for the author one of the prizes awarded by the Institute (Academy of Sciences) in 1850.‡ This two-fold monography has been so often quoted in the course of this discussion, opened at the Academy of Medicine, that it seems proper to give here an analytical account of it. The two essays possess further much true interest.

In proportion as our knowledge of Cretinism has been extended, as pathological observation has passed from travelling physicians to physicians resident in the Cretin districts, many analogies, striking at first sight, have disappeared before the light of a more mature examination. Unperceived differences have been discovered. The unity, something artificial of Cretinism, has not remained quite entire. In a geographical point of view, even authentic facts have been found to contradict the opinion so strongly founded, which ascribed the origin of the disease to the moist air of deep valleys. Cretins have been found in the plain country;‖ they have been met with in the midst of populations varying in aspect and in constitution. The definition of Cretinism requires to be extended, so as to comprehend those more precise and more numerous observations. Recent inquirers have recognised types in situations, in which the early authors had seen only degrees. It may be asked whether it be possible to explain by a lesion at all times identical incidents so dissimilar. The incidental affections of the Cretin, the very different nervous disorders to which he is subject, ought to lead to the supposition that the concomitant alterations had not the uniformity which they were originally believed to possess.

It is in this manner that there has been established a new tendency in inquiry, of which Stahl is evidently the representative most in advance. Not only does he admit species, but, proceeding still farther, he proposes the question, whether it be a matter of scientific necessity that we should recognise the existence of the genus Cretinism. What line of demarcation separates Cretins from the order of Idiots? Formerly the distinction was precise; at present, when observers have remarked so many varieties in the symptoms, physical and moral, it becomes artificial and unserviceable. This forms the originality, but it is also the vulnerable side of the work of Stahl. The descriptions, like the anatomico-pathological researches are taken from patients who do not, it may be said, belong to the same category; those best characterised leave doubts upon the correctness of the classification. It is accordingly, not without reason, that Rösch has declared that the Cretins observed by Stahl are essentially different from those observed in other places.* In point of fact, it is in a fertile country, and a country with few physical peculiarities, that the disease is developed, in the centre of a population, robust, notwithstanding fevers, well-fed, of intelligence, rather refined than obtuse, and the character of whom might perfectly well be compared with that of some of our own provinces (France). We have neither the number nor the geographical distribution of the individuals affected; and we have difficulty in explaining by mere forgetfulness the silence of the author upon a point of this importance.

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* Neue Untersuchungen über den Cretinismus, in 8vo, Zwey bande Erlangen, 1844.

† Beitrag zur Pathologie des Idiotismus endemicus in den Bezirken Salzheim und Gerolzhofen in Unterfranken.

‡ Neue Beitrage zur Physiognomik und Pathologischen Anatomie der Idiotia Endemica in 4to..pp. vi., und 77. Fig. Erlangen.

‖ Morel sur le Cretinism de Rosieres (Meurthe) 1851.

The object of Stahl was particularly to fill up two chasms in the history of Cretinism. He wished to complete our knowledge of what relates to the evolution of Cretinism and the method of cure, artificial or spontaneous; and especially to add to the little knowledge then possessed upon the subject of the pathological anatomy of the distemper. This last division is the most extensive; and it is that which alone at present claims attention.

The anatomical inspections of the bodies of Cretins have been for a long time a rare occurrence. When inquirers were tired with describing the three skulls in the museum at Pavia, they had recourse to charnel-house collections; in addition to the defect of information as to the history of skulls obtained from these receptacles, observers were thus obliged to confine their theories to the examination of deformities of the skull. The anatomical examination of dead bodies was deemed almost impossible among populations hostile and with little civilization. Joseph and Charles Wenzel express a doubt whether such proceedings be at any time practicable. Further, by reason even of the difficulties remarked by all writers, the opportunities which were furnished by accident, were carefully investigated; the isolated cases are in amount sufficiently numerous; and our poverty is more apparent than real. We have already cited the facts collected by Foderé and Iphofen. To these may be added those reported by Autenrieth, Seiler, Heyfelder, Rösch, Valentin, Wetzler, Jager. Stahl is the first who has collected observations on the bodies of the dead, either original, or at least not previously made known, in number sufficiently great to allow a comparison to be made.

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* 18eme Congres des Naturalistes et des Medecins Allemands.

We have already mentioned what was the leading idea of this physician, and in what manner he had rather exaggerated than diminished the varieties in the form of this disease. The effect of this is, that it is impossible to give a summary of his observations in short and positive conclusions. The measurements of the skull furnish merely elements of comparison. The whole conclusion which he deduces from them is to collect the cyphers in a synoptical table. We shall nevertheless endeavour to condense the summary by which he terminates the second Memoir, and which comprehends the result of twenty-three anatomical inspections recorded with detail in the course of the work.

1. *The Cranium.*—The bones are attenuated in certain spots, and thickened in others without any rule. The thickening may proceed to the length of hyperostosis with deformity of the *cranium*, incomplete destruction of the different orifices or *foramina*, obliteration of the canals. The most remarkable alterations are those which depend on an arrest of developement; for example, the petrous portion of the temporal bone is feeble and short; the cuneiform process of the occipital bone is in like manner too short; different bones remain more or less detached, as in the fœtus,—that is not united; the processes are in general rounded and effaced. The results of measurement are so variable that it is impossible to frame any law regarding the absolute and relative dimensions of the skull. The defect of symmetry constitutes one of the most frequent alterations. Prominence of one part of the cranial vault, with flattening of the parallel region, deviation or obliquity of the bones of the face, deformity of the different apertures, inequality of the digital impressions, of the crests, of the processes, are the principal. The rest of the skeleton is also the seat of very variable lesions.

This first part contains, as may be seen, few new documents; but it is impossible to doubt that the facts are better arranged than heretofore, and the alterations are appreciated at their just value.

2. *The Brain.*—The anomalies present not less diversity. The organ is diminished in volume, either totally or partially. The nerves, and especially the *medulla oblongata*, partake more or less in this deformity. The convolutions are either too deep, or not of the normal depth. The cerebral substance is hard or softened. In general, the cortical layer predominates. The ven-

ricles are void of symmetry, incompletely formed, or there are upplemental ventricles. Effusions of serum and hydatids are ery frequently observed.

There is probably enough of this sketch, rather concise than ncomplete, to show that the pathological anatomy of Cretinism s not exempt from the confusion in which the most part of ce-ebral lesions are situate. Meanwhile, it must be said, that imilar contradictions, so candidly enumerated, carried even to he length of a dogma, afford not satisfaction to all minds. The uestion has been proposed, whether, amidst those alterations, the diversity of which we have remarked, there be not some, which, predominating over the others, ought to be regarded as characteristic. M. Ferrus, in the discussion which he maintains with so much knowledge, and so much distinction, placed in the first rank the effusions taking place within and without the brain. He supported his doctrine upon the basis of a striking comparison between chronic Hydrocephalus and Cretinism. Of the memoir in which these ideas were enumerated, an account was given in a former volume of this Journal; and the debates to which they gave rise are so well known, that it is unnecessary to recal them at present.* The opinion which in Germany shows the tendency to become established, though it be not yet expressed in the terms of an exact formula, is that Cretinism is under the dependence of an œdema rather than of a cerebral dropsy.

The most recent work published upon the subject of cretinism, that, namely, of Dr Niepce, physician at the mineral waters of Allevard, contains the account of six instances of anatomical examination. The author is less devoted to the descriptive and anatomical sides of the question than to that of the origins of the disease, its mode of distribution, and its propagation in the valley of the Isere, in Savoy, in the departments of the Isere, of the Upper and Lower Alps.

In a pathological point of view, it is difficult at present to be conducted to new data. In an anatomical point of view, the author belongs, if such a term may be used, to the school of C. Stahl. He observes and establishes without system. The measurements of the cranium are collected in a synoptical table, comprehending eighty-three cases, without conclusions, and without other points of information, excepting those of the age and stature of the individuals. To measurements of this kind we attach not an extreme value. But, further, as the degree of their importance depends on their precision, it is difficult to believe that the skulls of living individuals, screened by coverings and hair, can be made available for such an investigation. Further, the numerical results obtained by Dr Niepce, though carried to the third decimal figure, appear to be blemished with some uncertainty.

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* See Medical Intelligence.

The result of the anatomical inspection has been analysed by the author, and the summary thus formed is here given.

Five cretins are distinguished in the following manner: *1st*, a male cretin, in the first degree, aged thirty-two years; *2d*, a male cretin in the highest degree, aged seventeen years; *3d*, a female cretin, in the second degree, aged twenty-three years; *4th*, a cretinous male, aged twenty-six years; *5th*, a cretin female, in the last degree, aged thirty-two years.

In the five inspections, M. Niepce found that the cerebral hemispheres were unequal; that the median fissure was strongly forced aside; and that the greater was the disproportion between the hemispheres, the more severe was the degree of Cretinism, and the less marked were the intellectual faculties.

M. Niepce found the convolutions and the anfractuosities of the brain less numerous and less deep than those of the brains of persons not Cretins. The anterior lobes presented little developement and were in proportion to the depression of the frontal bone. The bones of the skull were very thick and very hard. The dura mater, which was very dense, was strongly adherent to the pericranium of the cranial vault; the sinuses were full of black blood. The arachnoid membrane contained a large amount of serous fluid. The brain appeared to him more dense than usual in four subjects, more softened in one only. The ventricular cavities appeared to him larger than in the normal state, and to contain a large amount of serous fluid. The pituitary peduncle was small, while the pituitary body was more voluminous than natural. Thus it weighed two grammes, forty centigrammes; one gramme, twenty-seven centigrammes; one gramme, [illegible] centigrammes. The *corpora striata* and the optic *thalami* were [illegible]. The walls of the *septum lucidum* were [illegible]; its [illegible] contained a considerable quantity of serous fluid. The mammillary eminences were small but dense, softened in [illegible]. The [illegible] of the brain were slender. In one subject the [illegible] was very small; in another the [illegible] was scarcely indicated. In one of them the [illegible] were small; in another the *corpus callosum* was [illegible]. In a third subject the conarium or pineal gland presented a great degree of developement. Its antero-posterior diameter was [illegible] millimetres; its transverse diameter [illegible] millimetres.

The Cerebellum.—The *cerebellum* was small; its substance was firm in some, softened in another. The plates and *lamellæ* or leaflets were in small number, irregular in one subject; the hemispheres were at the same time unequal. This organ was flattened. In the fourth subject, the median lobe was very much developed, the ventricles were full of serous fluid. The occipital *fossæ* being almost completely effaced in Cretins, the lower surface of the *cerebellum* is modified in a corresponding degree and manner; the two lobes are flattened and the large fissure is shallow. In one subject the inferior vermiform body was scarcely indicated. The valve of Vieussens was very dense. The peduncles or crura of the *cerebellum* were very delicate.

The Isthmus of the Brain.—The *tubercula quadrigemina*, were large and irregular. The *corpora geniculata* were small.

The Spinal Bulb.—The Spinal Bulb always appeared smaller than natural. The Pyramidal bodies appeared to M. Niepce in some instances small, in others normal. The Olivary bodies presented the same anomalies. The basilar or cuneiform process being horizontal, the spinal bulb could not occupy the basilar groove, which does not possess the necessary degree of excavation; and the *medulla oblongata* cannot easily penetrate into the occipital *foramen.*

M. Niepce finally found that the complicated interlacement of the numerous nervous fibres, which is observed so well developed and distinctly perceptible in healthy individuals, had scarcely any existence in the Cretin annular protuberance, and that the spinal bulb was greatly smaller than it ought to be and softened.

The Medulla.—M. Niepce ascertained nothing abnormal in the *medulla*, except in its small size, and that it was immersed in a quantity of serous fluid more abundant than ordinary.

A fact which several authors had previously ascertained, he found, namely, that the *foramina lacera posteriora*, through which pass the *Nervi Vagi* and the *Nervi Glossopharyngei*, were almost obliterated and these nerves were very small. The other cranial nerves presented nothing abnomal.

Having examined carefully the auditory apparatus of one of the deaf and dumb Cretins, M. Niepce found that the diameters of the *meatuses* and of the apertures were much narrowed; that the *ossicula auditus* were spongy and larger than they generally are; the nervous pulp of the acoustic nerve was denser than it ought to be; the conduits were almost obliterated.

These anatomical results are, it may be observed, in accordance, upon almost all points, with those which have been formerly mentioned. The lesions present neither the unity which the earliest observers ascribed to them, nor the diversity which has since with some exaggeration been represented to prevail.

It was nevertheless impossible that Pathological Anatomy applied to Cretinism should not feel the re-action of the predominant opinions. The examination of the organs such as they are observed after life is extinct, the simple inspection of the solids, are no longer adequate to account for the alterations induced by the disease. The more a distemper is generalized, the more we are disposed to search for its character elsewhere than in a local or directly appreciable disorganization. Does Cretinism not present itself in such conditions that the new means of study are applicable to it with advantage? This is the question which in late years, some physicians have proposed for the first time, and which, led away by an apparent utility, they have even attempted to resolve.

Already Stahl in his first Memoir had attempted to obtain, from the chemical analysis of the Cretin brain, information which the other modes of investigation had failed to supply. This attempt was attended with so little success, that he was not encouraged to repeat it. Dr Niepce has devoted his attention to analyses of the blood, which, however, have led him to no conclusion. *Lastly*, still more recently, Dr Erlenmeyer, Director of the Institution for the treatment of Idiots and Children Imbecile in Mind, at Bondorf, near Coblentz, has chosen, for the subject of a long Memoir, the blood, the urine, and the alvine evacuations of patients of this description.* Besides that the classification of patients is sufficiently defective, in so far as it unites forms of disease very remote from each other, the analyses have added no satisfactory information to that which had been learned by other means of observation. It will be sufficient to name only some of the propositions which form the summary of the particular researches. The specific gravity of the blood is liable to great variations; the serum is in general clear and transparent. The microscopical examination brings to sight anomalies neither in the figure of the globules, nor in the other elements; the water appears to be in more considerable proportion than in sound persons. The solid parts are in quantity variable according to circumstances; the fibrin is found in amount nearly normal; the proportion of salts is little diminished.

It may be proper to mention that the description of two Cretin skulls is given by Dr Betz in the first number of the Journal of Rösch, and a note from the same author appears in the second number. These articles, however, contain nothing requiring particular notice at the present time.

In the Medical Intelligence will be found some observations on the influence and operation of the alleged causes of Cretinism and Goitre, in reference to the Inquiries and Researches of Drs Grange and Ferrus.

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* Beobachtungen über den Cretinismus, Journal de Roesch, 2d band. 1851.

ART. VI.—*On the Impregnation of the Ovum of the Amphibia. Section First.* By GEORGE NEWPORT, F.R.S., &c. (From the Philosophical Transactions of the Royal Society for 1851. Part I., p. 169.) Received 20th June.—Read 20th June 1850.

THE Amphibia, of all the vertebrated animals, afford to us the readiest means of investigating the difficult subject of Impregnation by actual experiment; and it is only, perhaps, by combining experiment with careful observations on the physical conditions that affect the development of the germ, and comparing these with the facts of the natural history and instincts of the species, that we may hope, ultimately, to obtain some further insight into this one of Nature's most hidden secrets.

I shall endeavour, therefore, in this communication, to show the condition of the ovum in the Amphibia through its earliest changes, and also before and immediately after impregnation, and to detail experiments made with a view to learn by what means its fecundation is effected;—and in a future communication I propose to trace the development of the embryo from the time of fecundation to that of its liberation from the ovum, in the two chief divisions of the class,—the tailless and the tailed Amphibia. The subjects thus naturally form two series—Impregnation and Development.

IMPREGNATION OF THE OVUM.

The history of what we can now prove to be the agent of impregnation, the spermatozoon, deserves to be especially noticed. Although great attention has been paid by physiologists during the last thirty years to almost every point of inquiry connected with the production and physical composition of the seminal fluid of animals, and its relation to the fecundation of the ovum, we have remained to the present time without any acknowledged proof either of the part which the different constituents of this fluid take in impregnation, or of the mode in which it effects impregnation. This perhaps is little to be wondered at when we remember how may years elapsed before the great discovery of Ham and Leewenhoek of the existence of moving bodies in the fluid, as part of its normal composition, was admitted. It is now one hundred and eighty-three years since Leewenhoek communicated the important discovery of these bodies to the Royal Society,* and regarded them as in some way essential to the fecundation of the ova; although his conjecture that they became the future embryos was erroneous. Eighty-five years afterwards, Dr Parsons, Foreign Secretary of the Royal Society, believed it to be "extreme nonsense to imagine that the insignificant animals called *spermatic animals* can contribute anything towards propagation," &c.,[*] and it was not until the publication of the observations of Ledermüller,[†] a few years after that, that the production of spermatozoa, as part of the fluid, began to be admitted. Dr Hill, in the notes to his English translation of Swammerdam's Biblia Naturæ,[‡] two years later, mentioned them as abundant in the frog at the season of pairing, but that it was then the fashion to doubt even their existence. Yet Needham, ten years after this, while acknowledging that these bodies are found in the fluid of all animals, adopting the views of Buffon and Daubenton, stated that they do not exist until after the fluid is removed from the vessels, and decomposition has commenced.[§] And later still, even in our own time, their existence has been denied in the most positive manner by Sir Everard Home.[‖] Spallanzani, however, was so well acquainted with them, as found in the frog and toad, that he has recorded his great surprise at not observing them in the latter on two occasions.[¶] Bonnet[**] and Gleichen,[††] also, well knew them to abound in the males of animals of distinct species at the season of impregnation, but discovered that they are usually absent in hybrids, a fact that has since been confirmed by Prevost and Dumas.[‡‡] These two observers, regarding the spermatic bodies, with Leewenhoek, as essential elements of the semen, believed that they actually penetrate bodily into the ovum, and become by metamorphosis part of the future embryo. Still more recently it has been stated by Dr Barry[§§] to this Society that he has actually seen the spermatozoon within the ovum, a statement which my own observations do not enable me to confirm.

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* Philosophical Transactious, 1667, vol. xii. p. 1040.

Before any satisfactory conclusion could be arrived at respecting the importance of the spermatic bodies in impregnation, it was necessary to ascertain their nature, to trace their mode of development and production, to establish the periods of their occurrence in different classes of animals, and to learn something of their chemical composition, as well also as of the fluid portion of the semen in which they move. Most of these inquiries have been well followed out by Wagner, Siebold, Muller, and more especially Kölliker, and more recently by Wagner and Leuckardt, from whose labours we have now some positive information which enables us to deduce a fair conclusion respecting their function, although a direct proof of its correctness is still to be supplied. Most observers now believe with Kölliker, that the spermatozoa (still so called) are not independent living organisms, but are merely elementary constituent parts of the male body, an opinion in which my own investigations lead me fully to coincide. This opinion, indeed, is not entirely new, as a like view was held by some observers at the beginning of the last century, when it was still questioned whether the spermatozoa are normal constituents of the semen. Dr Drake,[*] in his "New System of Anatomy," while acknowledging that he had seen the seminal animalcules, and combating on the one hand the theory of Leewenhoek respecting them, and on the other the view that had previously been held with regard to the ovum, doubted their separate organization, and suggested that they "may be nothing more than some large particles of mixed fluid, whose motions and different figure the microscope discovers to our eyes," &c. G. TREVIRANUS[†] more recently held a similar opinion, that they are not independent animals, but are analogous in their structure and properties to particles in the pollen of plants, and that their motion is of the kind discovered by ROBERT BROWN in vegetables. KÖLLIKER,[‡] however, first distinctly referred them to a class of known organic constituents of the living body, the vibratile cilia, a view which had previously been discussed and inclined to by MULLER.[§]

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[*] Philosophical Observations on the Analogy between the Propagation of Animals and that of Vegetables. 8vo. 1752 (note), p. 44.

[†] Physikalische Beobachtungen der Samenthierchen. Nuremb. 1756. And also, "Beyträge zu denen Beobachtungen deerer Saamenthiergen und Kleiste Aale gehörig. 12mo. Frankfurt und Leipzig, 1759."

[‡] Book of Nature, folio, part 2 (note). p. 105, 1758.

[§] Notes des Nouvelles Recherches sur les Découvertes Microscopiques de l'Abbé SPALLANZANI par M. NEEDHAM. Lond. 1769, tom. i. p. 196.

[‖] Lectures on Comparative Anatomy, 4to, vol. v. pp. 332 and 337. 1828.

[¶] Dissertations relative to the Natural History of Animals and Vegetables. Lond. 1789, p. 151.

[**] "Contemplations de la Nature;" and "Œuvres d'Histoire Naturelle," 4to, tom. iii. p. 454, &c. 1779.

[††] Abhandlung über der Samen-und Infusions thierchen. Nuremb. 1788.

[‡‡] Annales des Sciences Naturelles (Prem. Série), tom. i. p. 182, 1824.

[§§] Proceedings of the Royal Society, vol. iv p. 432. Phil. Trans. part 1, 1843, p. 33.

But however much our knowledge has become settled in regard to the nature of the spermatic bodies themselves, and their mode of development, their relation to the fluid portion of the semen in which they are contained is still a matter of doubt. H. Goodsir[‖] regards certain albuminous flakes in the fluid portions of the semen of Crustacea as the debris of dissolved cells, and as the source of nourishment and development of the spermatozoa; while a more recent observer, Dr Kirkes,[*] regards the spermatozoa as the elaborators of the fluid, and the conveyers of it to the ovum at the time of impregnation. This latter supposition was originally advocated by Wagner, Valentine and Bischoff. But two of these observers have recently changed their views,[†] and now regard the fluid portion as only of secondary importance in impregnation, and the spermatic bodies as of essential. This view, as Wagner states,[‡] is founded chiefly on the fact, that in some of the invertebrata the whole mass of the semen appears to be constituted almost or entirely of spermatozoa, while scarcely any *liquor seminis* can be detected;—and further, on the great improbability, perhaps impossibility, of the liquor seminis of those animals which expel their ova into water before impregnation being brought into contact with the ovum. But the same author justly remarks, that "even up to the present day this hypothesis of the influence of the liquor seminis has not met with any direct refutation." To this I may add, that however strong the presumption may be in favour of the agency of the spermatozoa in those instances in which the liquor seminis has not been observed it affords no sufficient reason for disbelieving that the spermatozoa are not resolved into fluid at the moment of fecundation; or that in those animals in which the liquor seminis occurs in abundance it is not that which impregnates the ovum.

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[*] New System of Anatomy, by JAMES DRAKE, M.D., F.R.S., vol. i. p. 352, 1707.

[†] TIEDEMANN, Zeitschrift, vol. v. part 2, 1835.

[‡] Beiträge zur Kenntniss der Geschlechtsverhältnisse-und der Samen flussigkeit wirbelloser Thiere. Berlin, 1841.

[§] Elements of Physiology (Eng. ed.) part 6, 1841, p. 1478.

[‖] Anatomical and Pathological Researches, 1844, p. 40.

The question then, so far as *proof* is concerned, both of the direct agency of the spermatozoa, and of the non-efficiency of the liquor seminis in impregnation, remains open, as well also as that which involves the knowledge as to how impregnation is effected.

It is to these questions that this communication which I have now the honour of laying before the Royal Society is chiefly directed. I propose *first* to show the time and mode of disappearance of the germinal vesicle, and the condition of the ovum in the Frog and Newt, immediately before and after impregnation, and to endeavour to supply proof from actual experiments that the spermatozoa alone, in all cases of communion of the sexes, are the sole agents in impregnating the ovum; and further, that impregnation *cannot* be effected by the *liquor seminis*; and next to examine in what way the ageney of the spermatozoa is influenced, impeded, or exerted.

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[*] Handbook of Physiology, 1848, p. 610.

[†] BISCHOFF in MULLER'S Archiv, 1847. WAGNER in Article "Semen," Cyclopædia of Anatomy and Physiology, part xxxvi., January 1849.

[‡] *Loc. cit.* p. 507.

1. CHANGES IN THE OVUM WITHIN THE BODY.

The ovum of the Amphibia has so frequently been the subject of examination by the best observers that a further detailed account of its development may at first appear to be useless, after what we already know of its changes through the labours of **Swammerdam**, **Leewenhoek**, **Roesel**, **Spallanzani**, **Prevost** and **Dumas**, **Rusconi**, **Baer**, **Reichert**, **Vogt**, **Bell** and others. But apart from the fact already mentioned, that the ovum of the Amphibia affords us the best means of actual experiment on impregnation, there are questions which relate to its earlier conditions on which the observers named are not agreed, but which are of importance with regard to the physiology of reproduction in the whole of the vertebrata.

I shall state, therefore, what I have myself observed with regard to these questions from the time when the ovarian ovum is approaching to maturity to that of its expulsion from the body, before entering on the subject of its impregnation.

As our means of comparing and testing the accuracy of all observations in natural history, and of experimental results in physiology, depend mainly on the correct identification of the objects examined, I may here state at once that the objects of the following details have been the Frog, *Rana temporaria*, and the Toad, *Bufo vulgaris*, among the **Anoura**; and the *Triton palustris*, *Lissotriton punctatus* and *L. palmipes*, among the Water Newts, the **Urodela**. Neither **Spallanzani**, **Prevost**, and **Dumas**, nor **Rusconi**, to whose observations I shall have frequent occasion to refer, mention the species they have examined. **Spallanzani** has given only the popular names of his animals, but **Rusconi** has given a figure of his, which appears to have been *Rana esculenta*, and as the description he has given of the ovum of this species agrees with the description given by **Prevost** and **Dumas**, it is probable that the species which they employed was the same.

The Ovarium Ovum.—When the frog, *Rana temporaria*, is examined in the autumn, after it has ceased to feed, and is preparing to retire to its winter quarters, the ova within it have already attained to more than two-thirds of their ultimate dimensions, before leaving the ovaries, and have begun to distend the abdomen. They remain in this state through the greater part of the winter, while the frog is hybernating, as Professor **Bell** remarks,[*] in the mud at the bottom of ponds and stagnant waters. But as the spring approaches, and the animal is aroused from its lethargy by increased temperature, the ova then rapidly acquire their full development. A female frog taken at the end of September, with her body enlarged with ova, was confined in water in a cold room, undisturbed through the winter excepting only at intervals of examination. The weather being mild, and the temperature of the room during October and November, being sometimes at or but little below 50° Fahr., the frog remained active, and came frequently to the surface to respire. In December the temperature sunk to below 40° Fahr., when the frog became lethargic, and scarcely changed its place at the bottom of the water during nearly a fortnight, while the temperature was almost stationary, and ranged only from 35° Fahr. to 37° Fahr. On the 10th of January, when it had again risen to 40° Fahr., and that of the water to 37°5 Fahr., the animal was still submerged and motionless; but on sudden exposure to the light of a candle it crept languidly, and almost imperceptibly, to a distance of about two inches, and again became quiet. On the 26th of January the temperature of the room had risen gradually and continuously to 52° Fahr., and that of the water to 48° Fahr, but the frog remained submerged and perfectly quiet with its eyes partially closed. Although it was not cognizant of any object by sight, the irritability of its body was now much increased, as it moved instantly when touched ever so lightly, but quickly relapsed into its previous state of rest. On the morning of the 1st of February, the temperature being then 48° Fahr., it was still submerged and motionless, but in the evening, when the warmth was increased to 52° Fahr., I found it with its nostrils only above the surface of the water, and evidently beginning to respire freely, but its eyes were completely closed. When the light of a candle was suddenly cast upon it, the eyes were slowly opened, but its body remained immoveable. On the following day, February 2, it was evident that its hybernation had been brought to a close, as it was then active, with its head out of the water, and its eyes widely open, and perfectly alive to external objects, as when the hand was slowly approached it withdrew beneath the water. The temperature at this time was 53° Fahr. During this period of hybernation the frog became slightly emaciated, and its abdomen, instead of being more enlarged, was somewhat diminished in bulk,—a good proof that during the inactivity of the respiratory and circulatory functions, the secretory also are lessened, and the development of the ova is arrested. But although no food was supplied to the frog at this time, and none probably is taken by the creature in its natural haunts, as at the time it comes forth but few of the objects on which it feeds are abroad, its body soon became enlarged, showing that the ova were then rapidly attaining their full development. Between the 2nd and 22nd of February the temperature of the oom was occasionally as low as 42° FAHR., yet the creature remained active beneath the water, without relapsing into its preious state of hybernation. It only continned longer beneath he surface without rising to respire. At this period, having ound that some frogs in their natural haunts had already come orth, I removed the subject of these observations also from the vater to a damp locality, and on the following day found it greatly changed in appearance. While confined in the water t was of a dull dirty brown colour, but some hours after its removal it cast its tegument, and changed to a bright yellow, with he usual brown markings, and had increased in size, both in ts body and limbs.

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[*] History of British Reptiles, 1832, p. 89.

The conclusion to which these circumstances seemed to lead vas, that quickly after the frog leaves its hybernaculum, it casts ts tegument as the insect escapes from its puparium, and acquires new vigour, while the ova are attaining their full growth. The Toad undergoes a similar change. About a fortnight ater in the season than the frog, I have seen many toads in a shallow ditch of slow moving water in the act of casting their dark brown tegument, and acquiring one of a greenish yellow.

On examining several frogs taken from their natural haunts, I found them in, as nearly as possible, the same state of development with regard to the ova, judging from external appearance, as the specimen I had watched through the winter A few had just paired, but the majority were still single. On opening the bodies of the latter, I found the ovaries greatly enlarged, and the ova, apparently ripe, but still contained in the ovisacs.

It is at this period, therefore, immediately after hybernation, and before the ova have left the ovaries, that the condition of the ovum is a matter of great interest with reference to the structure and contents of the germinal visicle, the period at which the vesicle is changed or disappears, and the circumstances under which the ovum escapes from the ovary, and is received into the oviduct.

The Germinal Vesicle.—The fate of the germinal vesicle in the matured ovum is still a matter of doubt. Previous to the embryological researches of Dr MARTIN BARRY, it was usually believed that the vesicle entirely disappears before or at the time of fecundation of the ovum. But this view was combated by the author named, who, quoting the opinions of previous inquirers, contended that the germinal vesicle in Mammalia does not disappear, as believed by PURKINJE,* in Birds and Amphibia, by bursting during the generative act, and pouring its contents into the germinal layer of the fecundated ovum.

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* Symbolæ ad Ovi Avium Historiam ante Incubationem, 1825; and Article "Ei," Encyclop. Wörterbuch, Band x. p. 112, 1834.

Neither, as supposed by **Baer**,* by being urged forwards and burst between the vitellus and its membrane before fecundation. Nor, as stated by **Rathke**,† with reference to the Crustacea, by disappearing while still within the ovary. Or, as supposed by **Wharton Jones**,‡ in Birds and Amphibia, by approaching the surface of the yelk, and by the giving way of the coats of the vesicle, and the effusion of its contents on the surrounding surface of the yelk. Or, further, as believed by **Bischoff**,§ by disappearing at the time of exit of the ovum from the ovary. On the contrary, Dr **Barry** has stated that the germinal vesicle returns to the centre of the ovum, and the germinal spot to the centre of the vesicle, before the ovum leaves the ovary, and that these do not become dissolved, but only are changed in character by a process of cell development within them, which ends in the production of two cells in the centre of the yelk, which are the foundation of the body of the future embryo.

Perhaps what I am about to mention may assist us to reconcile or correct the views of this embryologist, as well as those of the authorities he has quoted. It is well known, that if the ovum of the frog is examined before the yelk has attained to one-half of its ultimate dimensions, the germinal vesicle is distinctly visible as a large circular body near the centre of the yelk, apparently granular in its interior, and more opake than the yelk itself. In the centre of the vesicle its nucleus, the germinal spot, is then equally distinct. If the ovum is examined when it has nearly acquired its full dimensions, the vesicle is still found to exist, but, as compared with the size of the entire ovum, is relatively smaller than at earlier periods, and is recognized with more difficulty, owing chiefly to the yelk cells having both increased in number around it, and also acquired a darker colour. It is equally well known that all appearance of the vesicle is lost in the amphibia, as in birds, before the ovum is prepared for fecundation; but as to the way in which it disappears, or even as to the time, observers are not agreed.

Structure of the Germinal Vesicle.—Having collected a number of frogs that had recently left their hybernacula, and had not yet paired, I placed some in spirit for examination. On dissecting them afterwards, I found that in some the ova had nearly reached maturity, but had not left the ovary. The yelks being rendered firm by the spirit, I was able, by gentle pressure, to break open some of these beneath the microscope, without diffluence of the contents. The aggregated yelk cells were then seen to consist of two kinds; the one dark-coloured, which form the upper portion of the yelk, and which diminish in intensity of colour from brown or black at the circumference to a leaden or grey in the centre; the other, which forms the inferior portion, consisting of cells that become lighter coloured as they approach the surface. The whole form a mass, of nucleated cells of nearly uniform size, closely aggregated together. In the interior of the yelk, amidst the dark-coloured cells, and *much more near the surface of the dark* than that of the white portion of the ovum, I was surprised to find the germinal vesicle still entire, and of a somewhat oval, lenticular form, although irregularly compressed by the contraction of the whole egg in the spirit. It was of a dense white colour, and opake, from the action of the spirit, and was in striking contrast to the dark cells of the yelk, which adhered to its surface, and amidst which it lay imbedded like the kernel in a peach or apricot. On the surface of the black portion of the yelk was a minute orifice, already noticed by PREVOST and DUMAS,[*] the outlet of a canal that passes through this portion of the yelk to the germiual vesicle in its interior. This I believe to be the result of the yelk cells having only imperfectly closed around the germinal vesicle. It is however of some consequence in the future development of the ovum, as it is in this canal, that the cleavage of the yelk is commenced. The vitelline membrane was already formed, but I could not discover any orifice or perforation in it, either corresponding to the canal in the yelk, or to any other part of the surface. It is not possible to mistake the germinal vesicle for any portion of the lighter substance of the yelk,—first, from the fact that the vesicle at this period is of an intense opake, white colour, very different from that of the yelk substance,—next, from its being completely isolated from the lighter and imbedded in the dark substance,—and lastly, from its being still invested with a distinct envelope. On removing the vesicle, and examining it separately, first, as taken from the yelk, without crushing it, and next by gentle compression, and with the highest powers of the microscope, the interior was seen to be filled with secondary cells. Each of these, formed by a distinct envelope, *appeared* to contain other, or tertiary cells, and strongly reminded me of the developmental cells of the spermatozoa in the male organs, since these again seemed to contain granules, or quaternary cells. In the midst of the secondary cells I was able to distinguish, in the centre of the germinal vesicle, in some specimens, one or two cells of larger size than the rest, and which I regarded as the remains of the germinal spot, or central nucleus. In those ova which, from their size and general appearance, seemed to be the most mature, the peripheral series of cells within the germinal vesicle were of smaller diameter than those nearer to the centre, as if the earlier developed secondary cells had disappeared and liberated their contents. The cells of the dark portion of the yelk that adhered to the germinal vesicle were also nucleated, but were of much smaller size than the peripheral cells of this body. I must remark, however, that saving the fact noticed, of having seen in some vesicles one or two cells near the centre of larger size than the rest, I have not been able to distinguish any separate nucleus, or germinal spot, which seems, at this time, to have disappeared as a distinct body. The germinal vesicle thus near its maturity is a mother-cell that contains a multitude of daughter or secondary cells, each, apparently, including its own progeny.

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* "Lettre sur la Formation de l'Œuf," in Breschet, Repertorium, 1829.

† Untersuchungen ueber die Bildung und Entwickelung des Fluss Krebses, fol. Leipzig, 1829.

‡ Philosophical Transactions, 1837, part 2.

§ R. **Wagner**'s Lehrbuch, &c., 1839.

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[*] *Loct.* tom. ii. 104.

It is thus seen that the germinal vesicle exists in the ovum [illegible] period of impregnation. But in individuals [illegible] and in which the ova are [illegible] [illegible]

[illegible] as nearly mature as that of [illegible] I have been [illegible] [illegible] The dark portion of [illegible]

[illegible] the same mass [illegible] same structure, and situated in [illegible] passing from the middle of the surface of the darker coloured portion of the yelk through its substance to the vesicle, as in the Frog.

In the lesser Newts, *Lissotriton punctatus* and *L. palmipes*, the yelk is of a brown or liver colour on the future dorsal surface, with a white central spot, and of a white or pale straw colour on the ventral. In ripe ovarian ova of these species also I have found the germinal vesicle of the same white colour, and having the same structure as in the larger Triton and the Frog.

In each of these instances, not merely when the ovum is immature, but even when nearly ready to leave the ovisac, the germinal vesicle is situated in the interior of the yelk, and not immediately at the surface, and thus far the fact is in accordance with the observations of Dr Barry, that in the Mammalia the germinal vesicle is in the interior of the ovarian ovum, and does not disappear on the surface. But, nevertheless, it is not in the *centre* of the yelk; its place, as I have stated, is *excentric.*

Time of disappearance of the vesicle.—It is well known that in frogs and toads the ovum is never impregnated until after it has left the body, and consequently not until long after it has left the ovary, and the germinal vesicle has entirely disappeared. This disappearance does not, I think, take place at the instant the ovum is about to leave the ovisac, but a short time before, as I have found the ova of the Frog in one instance more matured than in that already described, still contained in the ovisacs, and yet most of them without any remains of the vesicle which I could identify as such, and with the yelks containing a larger proportion of white substance, as in the Toad. In a very few, however, the vesicle was still present, and exhibited the structure I have described. Certainly, then, the vesicle is not burst at the moment the egg escapes from the ovisac. There seems to be a short period of time between the disappearance of the vesicle and the full maturity of the ovum, during which the yelk itself undergoes some further change, and acquires the appearance noticed in the matured eggs of the Toad, and in the most advanced of those of the Frog. No trace of the germinal vesicle can be detected in any ova that have left the ovary and are contained in the cavity of the abdomen, before entering the oviducts. I have examined many ova, both of the Frog and Newt, from the abdominal cavity, but in every instance the vesicle has entirely disappeared. In some specimens, which seemed to have been in the act of escaping from the ovary when the animal was killed by immersion in spirit, I have found in the place occupied by the vesicle an aggregation of white nucleated cells, which, examined by the microscope, exhibited a close resemblance to those seen in the interior of the vesicle. In the midst of these there has occasionally been one or two of larger size than the rest, and which I have imagined to be the remains of the germinal spot, and possibly the origin of the future embryo vesicle of the impregnated ovum, an opinion, however, which I have not had the means of verifying, and I must further state that I have failed to recognise these larger cells in ova that were free in the cavity of the abdomen. Each of the three species of Newt, as well as the Frog, have presented similar appearances in the germinal vesicle and ovum under similar circumstances.

Thus it is quite certain that the germinal vesicle disappears in the Amphibia before the ovum enters the oviduct. I believe it does so in the interior of the yelk, not in the centre, but nearer to the dorsal than to the future ventral or white surface; and not, as has been supposed, on the dorsal or dark surface, between the vitellus and the vitelline membrane. This view is supported by the fact, that that portion of the yelk which incloses the vesicle in an advanced stage of the ovum in the Frog is of a more or less intense black colour, while the vesicle is perfectly white; and that at a further advanced stage, after the vesicle has disappeared, and its place is occupied by a collection of white cells, the dark portion of the yelk still preserves its intense black colour, except at the point that corresponds to the central canal, which then has a leaden hue. Prevost and Dumas,[*] and also Rusconi,[†] have mentioned that there is a yellow spot at a corresponding part of the dark surface of the egg of the species they have examined after impregnation, *Rana esculenta* ?, but these appearances must not be mistaken for the general vesicle arrived at the surface. There is a similar spot, and that too of an elevated form, on the egg of each of the *Lissotritons*. But independent of the fact that the germinal vesicle has entirely disappeared from the interior of the egg before it escapes from the ovary, this spot is shown not to be the vesicle, both in the fact that in the egg of *Rana temporaria* the dark portion of the yelk is unchanged, while in each case the spot is perforated, and leads into the canal that passed originally to the vesicle. I regard the spot as simply a protrusion outwards of the edges of the canal while closing, after the vesicle has disappeared. I shall presently show that a similar white spot is formed on the under surface of the egg of the Frog soon after deposition, and which might equally well be mistaken for the germinal vesicle.

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[*] Annales des Sciences Naturelles, tom ii. p. 104.

[†] Developpement de la Grenouille Commune, 4to. Milan, 1826, p. 9.

Mode of disappearance of the Vesicle.—The mode in which the vesicle disappears may be inferred from the facts of its structure. Being filled with a progeny of cells which we may regard as of different periods of growth, and these again containing others, it is fair to conclude that this process of cell-formation is that by which the parent vesicle is ultimately destroyed. At the time when the germinal vesicle has nearly attained its full size, the peripheral cells are smaller than those nearer to its centre, while the yelk cells that surround the vesicle are still smaller than either, and are of a dark colour. When, therefore, the vesicle has acquired its full size, by the simple vegetative endogenous growth of the contained cells, we may fairly presume that the death of the parent mother-cell, or germinal vesicle, takes place as the result of their enlargement, by the diffluence of its investing membrane; and the enclosed daughter cells, thus gradually set free in the midst of those of the yelk, as in the ideal, form one mass with the latter, and the moment of the actual disappearance of the vesicle thus escapes direct observation, its previous existence being indicated only by the unbroken outline of the investing membrane.

These views lead me to agree with WAGNER and BARRY in regard to the structure and mode of growth of the germinal vesicle, but not as to that by which it disappears. Dr BARRY indeed believes that the vesicle only becomes changed by its mode of development, and does not cease to exist. But most certainly it does disappear in the Amphibia, and, as I believe, through the growth of the young cells in its interior. I cannot therefore agree with Dr BARRY, that the changes in the vesicle end in the production of two cells in the centre of the yelk, that give immediate origin to the embryo; but rather believe that it is from one of the central cells of the germinal vesicle that the future *embryo vesicle* takes its origin, while the remainder of the liberated cells are distributed with this through the substance of the yelk, when the segmentation of this body takes place. This opinion is in accordance with that of VOGT,[*] who found that in the ova of *Alytes obstetricans* the germinal spots increase in number, and that a few hours after fecundation small vesicles, similar to these spots, are scattered through the yelk. I have myself found similar vesicles in the fecundated egg of the Frog about three hours after impregnation, but have not traced them to their origin. With regard to the disappearance of the spot in the germinal vesicle, the facts observed in the ova of the Frog agree with those noticed by KOLLIKER[†] in the ova of intestinal worms, that all appearance of the spot is lost before that of the vesicle. This circumstance, however, may be owing either to the spot having given origin to cells in the vesicle which quickly attain to similar dimensions, and from which it is not otherwise distinguished, or to its becoming entirely obscured by their multiplication. Although no observations have been made on the origin of the embryo vesicle that appears in the yelk after the disappearance of the germinal, I am still inclined to regard this as being in some way derived from the lost germinal spot, notwithstanding that KOLLIKER found a certain period of time elapse between the disappearance of the vesicle and that at which he was able to recognize this body.

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[*] Untersuchungen uber die Entwickelungsgeschichte der Geburtshelfer-Kraete (*Alytes obstetricans*), 4to, 1842.

[†] Müller's Archiv, 1843.

Transit of the Ovum.—Thus, then, when the ovum escapes from the ovisac and ovary into the cavity of the abdomen, the germinal vesicle and spot have entirely disappeared, and it consists only of the yelk enclosed in an exceedingly delicate, structureless, vitelline membrane. This is its condition in the Frog, Toad, and Newts. It is then extremely delicate and easily lacerated. The mode in which the ovum passes into the oviduct has been the subject of much inquiry. I am quite certain, as SWAMMERDAM long ago showed, that the ova, when mature, pass from the ovaries into the cavity of the abdomen, and from thence into the oviducts, in the Frogs, Toads, and Newts, quite independent of any intercourse with the male, as I have myself recently had an opportunity of proving. The frog I have already mentioned as having watched through its season of hybernation, was kept apart from all others until the 2d of April, at which time she had not deposited any ova. But from the altered form of her body it was evident that the ova had passed, or were at that time in the act of passing, into the oviducts. I then placed her in a vessel with others, some of which were males, but not one of these joined with her. Nevertheless, on the 6th of April she cast her ova, without having paired, and died on the following day. On examination after death, I found that the whole of the ova had left the ovaries, all of which, excepting only two still free in the cavity of the abdomen, had passed through the oviducts without any intercourse with the male. These ova of course were sterile; but it was worthy of note that their envelopes did not expand to so great an extent as those of the eggs of paired individuals.

Passage into the Oviduct.—In what way the ova pass from the cavity of the abdomen into the mouth of the duct has never been satisfactorily explained. SWAMMERDAM examined the question with much care,* but was unable to form any decided opinion respecting it, as he correctly states that the mouths of the ducts are at a distance from the ovaries, and are not free to grasp the ova like the fimbriated extremities of the Fallopian tubes in Mammalia, but are confined in the peritonæum, which is continuous with that which passes over the pericardium and heart. PREVOST and DUMAS have since stated* that the ova after leaving the ovaries are seized by the tubes ("sont saisis par des trompes;") but they do not show in what way this seizure is effected. They have omitted to describe the structure of the parts concerned in the act, and have not mentioned the way in which the ova are conveyed to the tubes from the ovaries, or whether the mouths of the tubes approach the ovaries, as in Mammalia,

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* Book of Nature, Part ii., pp. 108, 109.

The entrance to the tubes in the Frog, as SWAMMERDAM has correctly shown, is in the peritonæum at each side of the heart, and I have found it in nearly the same place in the Newts. The apex of the pericardinm in the Frog is attached to the cartilage of the sternum by two layers of peritonæum, which together form the mediastinum, and enclose between them the trunk of the median abdominal vein, a branch of the Vena Cava. Tracing one of these layers of peritonæum upwards and over the pericardium, we find in it an orifice, at the part where it is reflected on itself to form the lateral portion of the suspensory ligament of the liver. This orifice is elongated, oval and funnel-shaped, and, when dilated, forms a kind of pouch at the anterior boundary of the space or cavity between the liver and the heart, and laterally it is in free communication beneath the suspensory ligament with the common cavity of the abdomen. The oviduct commences in this dilated orifice, as a narrow tube with thick muscular parietes and with a thick mucous lining. It passes at first upwards and forwards, confined to the peritonæum, and then outwards above the base of the lung, gradually increasing in its dimensions. Immediately after it has passed the lung, it becomes more enlarged, and as it passes backwards to the side of the spine forms many convolutions, which end in the dilated oviduct or common receptacle for the eggs ready to be deposited.

The commencement of the oviduct in the Newts is very similar to that of the Frog. In the *Triton palustris* it differs only in the entrance being larger, and situated more to the side of the body, and above the lung, to the base of which, as well as to the peritonæal investment of the heart, it is confined, as in the Frog; but it has a more free communication laterally with the common cavity of the abdomen than in that animal.

The ova escape from the ovaries into the cavity of the abdomen among the viscera both in the Frogs and Newts, and are carried forwards to the spaces between the liver and heart on each side to the dilated mouth of the oviduct. They certainly are not seized by the tubes as they escape from the ovaries, as they are constantly found free in the abdominal cavity, while the mouths of the tubes being confined in the peritonæum and having no appendages, cannot be extended to reach them. Their transfer seems to be effected in the Frog in part by the action of the abdominal muscles forcing them onwards in the spaces between the viscera, aided perhaps by the peristaltic action of the stomach and intestines; and their entrance into the tubes, when arrived in the vicinity, seems to be induced by an ingurgitory or suction action at the mouth, occasioned by the alternating and pulsatory motion of the heart, with which the tube is connected by means of the peritonæum. The tube itself is formed of strong longitudinal and transverse fibres, which are continued into the peritonæum, and the former especially into the suspensory ligament, the free external margin of which bounds the outer side of the orifice. The transverse fibres are strongly marked at the commencement of the orifice, where there is a slight pouch; so that when the eggs are entering, these fibres doubtless prevent their return and transfer them onwards.

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* *Loc. cit.* tom. ii. p. 105.

This, I believe, is the way, in which the eggs enter the oviducts. It is quite certain from the anatomy of the parts, that they cannot be grasped by the oviducts until they are conveyed to them. I have not actually witnessed the passing of the eggs from the abdomen into the ducts in the Frog, but I have seen the eggs moved onwards in the smaller Newt, *Lissotriton palmipes*. Having deprived a female of this species of sensation and power of motion, by division of the spinal cord through the medulla oblongata, I proceeded to open the abdomen to obtain ova from the oviducts for experiments on artificial impregnation. I then found, that a number of ova were free in the abdominal cavity, and that some had very recently entered the ducts, while others were in the immediate vicinity of the mouths. The heart was still pulsating vigorously and with great regularity; and I then saw that at each pulsatory action the ova passed slowly forward between the liver and lung, towards the mouth of the oviduct, which still contained two or three ova that appeared to have entered at the moment of the operation. I did not witness the actual entrance of an ovum, but saw that the action of the heart certainly had the effect of inducing the advance of it to the mouth of the tube, and quite sufficient to lead me to regard this as one of the chief means of its entrance into the duct.

It is not until the ovum has become clothed in the oviduct with its gelatinous envelope that it is susceptible of impregnation. This remark applies equally to the Frogs, Toads, and Newts. The ova of the Frog and Newt at large in the abdominal cavity are always entirely without this envelope, and consist simply of the yelk mass enclosed in an extremely delicate vitelline membrane. They are so easily injured that it is only with great difficulty that they can be removed from the abdomen for examination unbroken. Those of the Newt, when taken up ever so carefully by means of a hair pencil, often burst the membrane simply by their own weight. But immediately after they have entered the oviduct and begin to acquire their envelopes, the yelk appears to undergo some change, as it becomes much firmer and is less easily injured. Shortly after the egg has entered the duct, it gains the first layer of an investment, which, from the great similarity it bears to the gelatinous layer gained by the ovum of the Rabbit in the Fallopian tube, and regarded by its discoverer in that animal, Mr WHARTON JONES,[*] as the origin of the chorion, I am disposed, with him, to look upon as the analogue of that layer. This covering adheres very closely to the vitelline membrane, and is scarcely to be distinguished from it, except at certain periods of change. It is acquired before the egg has arrived at the first convolutions of the oviduct. During the remainder of its passage the egg gains two other distinct layers of similar investment, which, together, we afterwards recognise as the gelatinous envelope of the Frog and Toad, and the capsule of the Newts.

These envelopes are not merely simple means of protection to the egg during the production of the embryo, as has been supposed, but, as I shall presently show, are essential to it at the period of fecundation, and without which the egg is not susceptible of impregnation. The layer of envelope which I regard as the foundation of the *chorion*, is a dense, but very transparent thin covering, in immediate contact with the vitelline membrane, and is formed of cells so closely aggregated together as to have coalesced into a fibrous structure. The two layers external to this are also formed of cells, which, with their nuclei, are distinctly visible in the envelope of *Triton palustris*, in which they alternate in regular series. Although these layers, which constitute the jelly in the egg of the Frog, become detached in that of the Newts quickly after oviposition, and, expanding as in the Frog, they leave the egg at liberty in a chamber in their interior, they are nevertheless essential to the impregnation of the ovum, which takes place before or at the time of leaving the body, as in Frogs and Toads. RUSCONI[†] removed the envelopes of the egg of frogs, and found that the embryo still became developed, and thence concluded that these coverings serve only mechanical purposes during the changes: but it will presently be seen that they have a more important function at a much earlier period. During the time they are in course of formation around the egg the yelk undergoes some further change. The light portion becomes of a whiter, and the dark portion of a deeper colour. Internally the cells vary more in size, the lighter-coloured being the largest. I have not succeeded in recognizing any embryo or central vesicle up to this period.

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[*] Phil. Trans. part 2, 1837.

[†] Développement de la Grenouille commune. Milan, 1826.

2. CHANGES AFTER SPAWNING AND IMPREGNATION.

First Period of Development.—It has been long known that a division or cleavage of the yelk of the Frog's egg is one of the earliest and apparently invariable results of fecundation. The primary division was first seen by Swammerdam, and was figured[*] and mentioned, but was not understood by him. Spallanzani long afterwards recognized it in the egg of the Toad (*Alytes obstetricans*[†]), which he says becomes about a day after fecundation marked "with two furrows which meet to form an angle,"—that the furrows afterwards become deeper, and that "two small tumours arise on each side of the furrows," —changes which have since been more accurately and completely described by Vogt[‡]. Spallanzani also says that the egg of the common Toad is "marked with four furrows which intersect each other at right angles nearly like the husk of a chestnut half opened," but he seems to have thought this was the usual condition of the ovum. To Prevost and Dumas,[§] however, we owe the important discovery of the cleavage of the yelk as a process of the fecundated egg; to Rusconi,[||] Baer[¶] and others, its full exemplification in the Amphibia; and to Barry[**] and Bischoff[††] its detection and elucidation in the Mammalia.

The agent immediately concerned in these changes is believed to be the embryo vesicle and its progeny, produced after the disappearance of the germinal vesicle. But it is yet uncertain what is the origin of the embryo vesicle, or whether it exists in the unfecundated ovum. As cleavage of the yelk certainly is not the result of the disappearance of the germinal vesicle, which disappears from all ova of the Amphibia, whether hey are afterwards impregnated or not, I was desirous, at the ommencement of my experiments on impregnation, to learn *rom direct observation* whether the unfecundated ovum ever asses through any stage of cleavage; since the ascertainment f the fact in the negative would be an important test in the xperiments I was about to make. For this purpose it was ecessary to collect many pairs of frogs at the proper season, nd when, from symptoms which are soon recognized, it was und they were about to cast their ova, to wait patiently, per- aps for many hours, for the result, in order that the exact ndition of the ovum, impregnated naturally, should be first certained. SWAMMERDAM long ago remarked that the awning of the frog takes place very rapidly "by a single fort."* It is often completed, as I have found in the English ecies, in a few seconds, and usually in less than a minute, ring which the male impregnates them, so that if the animals e not closely watched, the opportunity of observing the ear- ist appearances of the ovum is lost. Having noted the con- tion of the impregnated ova of several pairs of frogs within e first few minutes after spawning, I found those of different dividuals vary much with respect to the white or inferior rface, and exhibit appearances that may readily be mistaken r the breaking up of a vesicle on the surface. This appear- ce is due to a more or less complete state of maturity of the gs of different broods, and according as their spawning has en retarded or hastened. The peculiarities are the most rked in the least matured, the white surface of the egg ing the last completed part, and forming the base of the egg the ovisac. Having noticed the appearances of the eggs en impregnated naturally, I was enabled to compare them th others impregnated artificially, and these with some of the ne brood not impregnated.

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[*] *Loc. cit.* tab. xlviii. figs. 5, 8.

[†] *Loc. cit.* vol. ii. p. 159.

[‡] Untersuchungen über die Entwicklungsgeschichte der Geburtshelfer-Kröte (*Alytes obstetricans*), 1842.

[§] Annales des Sc. Nat. tom. ii. p. 110, 1824.

[||] *Loc cit.*

[¶] Muller's Archiv, 1834.

[**] Philosophical Transactions, 1839, 1840.

[††] Entwickelungsgeschichte des Kaninchen-eies, 1842.

Immediately after the frog has spawned, the ova form a close unded mass, which at first is scarcely so large as a walnut. hey then seem to consist almost entirely of dark-coloured lks with thin gelatinous envelopes. The form of the egg is en somewhat oval, with the white portion a little more conical an the dark, and differing slightly in different ova. In some ere is a dark spot in the centre of the white, that looks like depression or cavity, or perhaps a vesicle. I am not certain at this appearance is in reality a vesicle, and therefore am ntent to describe it as a spot, although it conveys the idea being a vesicle. In some ova there are two, four, or six of se spots imbedded each in a small portion of white substance.

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* *Loc. cit.* part 2, p. 111.

When only a single spot exists, the white surface of the egg for some space around it is more defined than afterwards, and exhibits faint indications of a crucial division of the yelk on this surface immediately around the spot. This is the condition of a few ova immediately after spawning; but the majority have then advanced farther in their changes, and show four or six rounded dark-coloured spots at a little distance in the place of the single central one. When four spots occur they are usually arranged in a quadrangle, and are less than their own diameter apart. They convey the idea of being derived from the central one: but I have never seen any division of this, and if such division takes place, I think it must occur before or at the very moment the ova are expelled. In a further advanced stage of the ovum the four dark spots have become larger, and are each imbedded in a distinct portion of the white surface.

One minute after deposition the spots are more widely separated, and are then each encircled by a separate patch of white substance. *Two minutes* after spawning six dark spots have made their appearance, one of which is situated nearly in the centre, and the remaining five are so arranged around this that the white patches in which they were imbedded seem to have coalesced. In *three minutes* the spots are further enlarged, and appear joined by a dark line of colour extending from each, so that the whole form, as it were, a knotted ring that includes a patch of the white surface of the yelk with one of the dark spots near the centre. In *four minutes* the ring around the included white substance is more distinct, and the white surface of the egg has increased in extent. In a further advanced stage at this period the white portion included in the ring exhibits the appearance of a white, very opake patch, the dark spot in the centre having disappeared. Around this opake white patch is the dark-coloured knotted ring, now become more uniform, and resembling a ragged chink or slight circular furrow or division in the white surface. The centre of this hemisphere of the egg thus comes to be occupied by a white patch instead of the dark spot. At *five minutes* this central white patch,—which, as before stated, and from what afterwards occurs, may readily be mistaken, on casual inspection, for the germinal vesicle, altered in its appearance and arrived at the surface,—were it not that we now know that this has long before entirely disappeared,—becomes more defined, and the dark circle around it is more uniform and distinct. At *ten minutes* the central patch is a little reduced in size, and the circle that encloses it begins to take the appearance of a diffused halo. At *fifteen minutes* the central white patch is more reduced, and the halo is spread wider, while the whole of this hemisphere of the egg has acquired a whiter appearance, and become more distinct from the dark colour of the sides and future dorsal hemisphere. At *twenty minutes* the central patch has become still smaller and rounder, and the dark halo much broader.

At this period an interesting circumstance occurs, which may hereafter be found to have some reference to changes in the interior of the yelk, possibly to some rapid evolution of the so-called central or embryo vesicle in the locality originally occupied by the germinal vesicle and spot, which, as we have seen, is nearest to the dark surface. This is the partial rotation of the entire yelk. Up to about this period the ova remain undisturbed in the water in a mass as they are expelled, and lie indiscriminately, some with the dark and some with the white portion of the yelk uppermost, or horizontal. But during the time that has passed since the ova have been in contact with water, the envelopes have imbibed fluid and expanded until these investments of the yelk have acquired a thickness equal to about two-thirds of the diameter of the yelk itself. The yelks, that have remained to this time with their white surface uppermost, now change their position spontaneously by a partial rotation of the whole mass of each on its axis, within the vitelline membrane, until the dark surface of the whole is placed uppermost. Whether this change of position is merely the result of an expansion of the vitelline membrane at this period, when the ovum is rapidly ceasing to be susceptible of impregnation, as I shall presently show is the case after this lapse of time in the water, or whether it be also connected, as we may fairly believe, with changes going on in the interior of the yelk, I am not prepared to decide. It is important, however, to note, that the change takes place at about the time, at which I have found a great abundance of bright clear rounded vesicles distributed throughout the yelk, but chiefly in the place originally occupied by the germinal vesicle. In some of these vesicles, which I regard as the progeny of the germinal vesicle, I have seen irregular-shaped nuclei that appeared to be formed of a multitude of nucleoli. These vesicles convey to me the same idea as that conveyed by the vesicles seen by Bischoff in mammalian ova, excepting only that in the egg of the Frog they contain compound nuclei.

At *thirty minutes* the central patch on the white surface of the egg has almost disappeared, and the halo around it is still more diffused. At *forty-five minutes* it has entirely disappeared in most specimens, and its place is occupied by a broad dark area which includes the boundary of the previous halo, and which appears to be occasioned by a slight depression in the centre of this surface of the yelk. *One hour* after spawning this depression is somewhat deeper. The white surface has become still more defined, and the dark has acquired a more intensely black colour. The egg remains in this state without further perceptible change during the succeeding *second* and *third* hour, excepting only that the depression in the white surface becomes a little deeper, but it has almost disappeared at the end of the *fourth* hour, when *segmentation* or cleavage of the yelk is about to take place. But this is not invariably the case. When it does remain it is always of an oval form, and the primary cleavage of the yelk, as it proceeds on either side, from above downwards, meets in its centre and invariably passes through it transversely to its long diameter. These are the first perceptible changes in ova that are impregnated by the natural union of the sexes, and when spawning has not been retarded. But in some broods of eggs that have been retained longer than usual in the oviducts, the whole of these changes have already taken place, in so far as regards those of the yelk, the white surface of which then exhibits an uniform appearance.

Changes immediately before segmentation or cleavage of the yelk.—Segmentation usually commences in from four to five hours. At about *one hour and a half* after spawning, the peripheral layer of cells on the middle of the dark or uppermost portion of the yelk of the impregnated ovum becomes separated from the inner surface of the vitelline membrane, and this separation goes on until a broad free space is left between this envelope and the superior layer of yelk-cells. This space, which we may designate the *respiratory chamber*, is at first but a small area above the middle of the dark surface of the yelk, and is commenced above the central canal. It seems to be occasioned by a recedence towards the interior, or a shrinking, at this period, of the yelk cells of the dark hemisphere of the egg, commencing in the centre of this part and extending gradually, but in a less degree, to the circumference. This recedence goes on until the space left between the vitelline membrane and the yelk is equal to about one-sixth of the diameter of the whole mass, when the space appears to be occupied by a very transparent fluid, interposed between the now depressed surface of the yelk and the vitelline membrane. In the centre of the black surface is the minute orifice noticed by **Prevost** and **Dumas**,[*] and **Baer**,[†] which leads into the central canal that communicated with the germinal vesicle in the ovarian ovum. It is in the margins of this canal that segmentation is commenced. While the space or chamber between the black portion of the yelk and the vitelline membrane is being formed, and from fifteen to thirty minutes before there is any sign of cleavage, the yelk becomes extended horizontally in a direction transverse to that in which the first cleft afterwards takes place, and assumes a transitory obtuse oval form, which it retains until the yelk begins to divide. The division, as correctly shown by BAER,* commences in the extension in opposite directions of at first a faint indentation in the margin of the central canal, which quickly becomes deeper, and is carried across the surface of the yelk, and gradually more and more deepening and widening as it proceeds, is carried round the sides, and meeting in the middle of the depression on the under surface, or of the remains of the white patch when this has not already disappeared, is completed by passing through the middle of the yelk ; which is thus divided into two portions. This first division occupies from twenty to thirty minutes before it is finished, and it is not until then that a second fissure is commenced.

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[*] *Loc. cit.* Tom. ii. p. 104, 1824.

[†] **Muller's** Archiv, 1834.

I have not had any opportunity of proving whether this division is the direct result of subdivision of the central vesicle, and the attraction of the yelk-cells in equal proportions around each division of that body, as believed by KOLLIKER,† and, as it seems fair to infer, is the case; but in addition to the observation by Professor SHARPEY,‡ that the contraction of the entire yelk at the commencement of these changes, and the movements he has observed among its granules as they proceed, are in favour of this opinion. I may remark that the extension of the yelk of the Frog's egg in a direction transverse to the first cleavage, may also be advanced as conformable to the same view. It is further supported by the fact which I have seen in the egg both of the Frog and Newts, that before the second or crucial cleavage is commenced the yelk becomes contracted, so that the first cleft for a time is almost imperceptible while it is extended in the transverse direction, or line of axis of the first division, after which the second or crucial cleft is commenced. To this I may add another fact which appears to be equally significant, and which occurs in the unimpregnated eggs both of the Frog and Newts, but more especially in the latter. Although no recedence of the yelk from the vitelline membrane takes place in the unimpregnated egg of the Frog, it becomes, nevertheless, slightly oval after the first few hours, but returns to its original shape some time afterwards. But the unimpregnated egg§ of the Newts is not only separated from the vitelline membrane, but also is depressed, and has a distinct pit in the centre of its upper surface, and also assumes an obtuse oval form, both which it retains, when the egg is preserved in water, until decomposition has commenced.

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* *Loc. cit.*

† MULLER's Archiv, 1843.

‡ QUAIN's Anat. fifth edition, 1848.

§ Or, possibly, partially impregnated.

Changes in the impregnated and unimpregnated Ovum compared.—Having traced the egg impregnated by natural union of the sexes through its first period of development, I was able to compare the phases it exhibits with those of the artificially impregnated, and these with the appearances in unimpregnated eggs of the same brood, placed under precisely similar circumstances with reference to light, heat, air, water, and locality. The ova experimented on were all procured from the same female, and the seminal fluid from the male with which she was paired, and at the time the female was about to spawn.

SPALLANZANI obtained unimpregnated eggs of the Frog for his experiments by opening the body of the female and removing them from the distended oviducts. This mode is exposed to the objection, that in the removal of the ova they are liable to be brought into contact with the blood of the animal from the cut vessels, and that the ova thus obtained may not be the most mature, and fitted for experiment. It seemed desirable, therefore, to obtain them by another mode,—the total withdrawal of the influence of sensation, and power of tension in the muscles by division of the spinal cord through the medulla oblongata. The attempt was made with a female frog that had been paired for several days, and, from appearances, would have deposited her ova naturally in the course of a few hours. The spinal cord was divided as quickly as possible with a strong pair of scissors immediately behind the brain, and this organ was also destroyed, so that all consciousness was annihilated. The attempt was successful. The muscles deprived of voluntary power instantly became relaxed, and allowed of the ova being passed, by gentle compression of the body, through the natural passage without contact with the blood of the animal, in greater or smaller number at pleasure, and thus afforded easy means of experiment.

I may once for all state that it was in this way that the ova were always obtained in the following investigations.

Recourse was had to a similar expedient to procure the seminal fluid from the male. SPALLANZANI had obtained the fluid, by vivisection, from the seminal vesicles themselves. But there seemed more objection to the adoption of this mode with the male than the female, the certainty of much of the fluid being lost, independent of the severity of the operation. Indeed SPALLANZANI states, that he was never able to procure more than from two to three grains from a single individual. I therefore vailed myself of a habit in the male frog, and which SPALLAN- ANI had previously noticed and taken similar advantage of in he newt, to obtain the fluid in greater quantities than by the node constantly adopted by that physiologist. When the male rog, like the newt, is taken in the hand, or slightly compressed, t the season of pairing, a quantity of fluid is immediately passed. This consists chiefly of seminal fluid mixed with water expelled rom the effect of the compression, or during the efforts to scape, as water is passed by other animals at the moment of apture. It abounds with spermatozoa in their most active state, nd thus is fitted for experiment. It required therefore only to ecure the limbs of the animal and compress it slightly, to obtain the fluid without severe injury. This ready mode was dopted on all occasions when the fluid was required, and the precaution taken always to examine a portion with the microscope, to be assured of its nature before employing it. Spermatozoa have never, during the season of pairing, been absent from it. At the end of the season they have been less abundant, and spermatozoal cells in greater proportion than at an early period. But in these cases I had reason to think that the chief part of the fluid consisted of water. It is probable that this was the case in the two instances of apparent absence of spermatozoa in the Toad, mentioned by SPALLANZANI,* and that the fluid did really contain spermatozoa, although few in number, and consequently easily overlooked, and that the ova were impregnated by these, and not by the fluid portion of the semen, as he appears to have supposed.

On comparing the white surface of the yelk of the unimpregnated with that of the impregnated egg, whether the egg had been fecundated naturally or artificially, I was not able to detect any difference during the first twelve minutes. The changes went on in both, and appeared to be almost identical in each. But after the time specified no further progress was perceptible in the unimpregnated ovum, which continued to exhibit the same appearance for several hours. But the white surface of the impregnated egg became more and more changed, up to the time of cleavage of the yelk, when it was almost an uniform surface.

These observations were afterwards repeated with similar results, and the conclusion to which they led was, that changes take place in the yelk from the period when the germinal vesicle disappears and the ovum leaves the ovary to the moment of its expulsion from the body, and which changes may proceed for some time afterwards quite independent of impregnation; and that these have some reference to the evolution of the central or embryo vesicle: possibly also that they do not cease immediately, but subside gradually when the stimulus imparted by impregnation is not supplied.

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* Dissertations relative to the Natural History of Animals and Vegetables, 1789, vol. ii., p. 151.

The experiments made to ascertain whether the unimpregnated ovum passes through any stage of cleavage, consisted of four sets, placed in four vessels of equal size, containing each about two ounces of water. Ova were passed from the same female as quickly as possible at the same time into each of these vessels. To one of these marked A, a considerable quantity of a mixture of seminal fluid, one part to three parts of water, was immediately added ; to a second, B, only a single drop of this mixture; while the third and fourth, C and D, contained only water with the unimpregnated ova, and the four vessels were then placed in every other respect under precisely similar conditions.

A few minutes after the impregnating fluid had been added to A, I examined some of the ova beneath the microscope, and found a vast abundance of spermatozoa adhering to every part of the surface of their gelatinous envelopes. On other ova from the set B, there were also many spermatozoa attached, but in much smaller number than on the ova of set A.

In *five hours and fifteen minutes*, the temperature of the room during the interval having ranged only from 53° FAHR. to 54° FAHR., segmentation had commenced vigorously, and was strongly marked in the whole of set A. But it had not commenced in set B. It did not occur in these until *five hours and twenty-two minutes*, when it began in these also. Thus there were *seven minutes'* difference in the commencement of the changes in these two sets of ova, a circumstance which led to the belief that this difference might have some reference to the relative quantities of the impregnating fluid employed,—an opinion which I had long before been led to by observations on the impregnation of the common Earwig, *Forficula*, in which it had appeared to me that deficiency in the quantity of the impregnating fluid is unfavourable to fecundation.

No segmentation or cleavage of the yelk took place in the sets of ova marked C and D, which, except in becoming a little oval, as already mentioned of unimpregnated eggs, remained, in so far as the appearance of the yelk surface was concerned, in the same state as at a few minutes after spawning, and they continued in exactly the same condition at the end of twenty-two hours. This I have since found constantly to be the case with unimpregnated ova, whether they happen to be exposed to a high or low temperature of the surrounding medium. The yelk of the impregnated egg gradually acquires a more intense black colour, which strikingly contrasts with the dull colour of the unimpregnated.

At the end of *six days* the majority of the ova in A and B were producing embryos, while those in C and D were fast decomposing.

These trials afforded the positive test I required from direct observation, as a fixed point in the experiments about to be commenced,—that segmentation certainly does not take place in the unimpregnated ovum.

3. SUSCEPTIBILITY OF THE OVUM.

I was not aware, at the time of commencing my experiments in March 1849, nor indeed until very recently, of the extent to which the original investigations of SPALLANZANI, and of PREVOST and DUMAS had long ago been carried,[*] and it has only been since my experiments were completed, and during the preparation of this paper for presentation to the Royal Society, I have learned by careful reference to their first memoirs, that they have anticipated me in part of this inquiry; that of endeavouring to separate the spermatozoa by filtration from the more fluid portion of semen, and testing the effect of these two constituents in artificial impregnation. To them therefore be all honour for the result; although even they, as they honourably mention, had themselves been anticipated in this by SPALLANZANI, and that too with similar success. The extraordinary results obtained by SPALLANZANI[†] in artificial impregnation, and the imperfect knowledge which we possess of the nature of the means by which it is effected, has induced me to endeavour to repeat and vary his experiment, and to conceive others, which, so far as I am aware, have not yet been attempted. I have been the more urged to this from the circumstance mentioned by SPALLANZANI, and already alluded to (p. 373), the occasional supposed absence of spermatozoa from fluid that is capable of fecundating ; and also from a belief formed long ago with regard to the Articulata, that the spermatozoa, nevertheless, certainly are the efficient agents in impregnation, although full proof of the fact has been wanted. I have been desirous therefore of learning how far this belief can bear the test of direct experiment, or the fact be capable of demonstration by artificial means in the Amphibia. As, however, the experiments I have myself made vary from those of the authors mentioned,—have not been influenced by the result they had previously arrived at,—have been somewhat more extended, and, as I believe, will now tend to place the fact of the direct agency of the spermatozoa in impregnating the ovum beyond doubt,—it has seemed desirable still to give them in detail, as assisting to establish an important point of knowledge by facilitating a comparison of the results of independent investigations.

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[*] Annales des Sciences Naturelles, Tom. i. et ii. 1824.

[†] Dissertations, &c. vol. ii.

Duration of susceptibility.—The length of time during which the ovum, after it has been passed, remains susceptible of fecundation, is affected by several circumstances. I had reason to believe at the commencement of my experiment that this time is very short. SPALLANZANI found that when the egg of the toad was expelled into water it was not susceptible of fecundation after a lapse of fifteen minutes.[*] This was at a raised temperature of the atmosphere, 81°.5 FAHR. On the other hand, he also found that, at this temperature, ova retained fourteen hours within the body of the female after death, and of course not in contact with air or water, might still be fecundated; and that when preserved in an ice-house fecundation might be effected at two days after the death of the parent.[†] But PREVOST and DUMAS arrived at the conclusion[‡] that the time is much more extended in the Frog:—that fecundation may take place when the temperature ranges from 53° FAHR. to 59° FAHR. at the expiration of one hour after immersion of the eggs;—that some eggs are fecundated at two, and a very few even at the end of three hours, after which no fecundation takes place. The results obtained by myself both on the Frog and Toad have been most in accordance with those by SPALLANZANI. This is the more worthy of notice from the circumstance that a slight difference between his and mine is readily accounted for by a corresponding slight difference of temperature, which SPALLANZANI, and PREVOST and DUMAS, have remarked, and since them also Mr BELL,[§] has great influence on the changes of the eggs and young. The temperature at which my test experiments were made, was a little lower even than that at which PREVOST and DUMAS made theirs; and yet I was not able to find any ova susceptible of fecundation after they had remained from thirty to forty minutes in water.

On careful examination of PREVOST and DUMAS' experiments, I think the difference may perhaps be due to a circumstance which seems equally to affect some of SPALLANZANI's results, namely, the mode in which the impregnating fluid employed was obtained. These authors state that the fluid they employed was expressed from the testicles of the frogs, so that from what we now know of the mode of origin of the spermatozoa, this fluid in all pro-

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[*] [illegible] p. 175.

[†] Ibid. pp. 176 and 177.

[‡] Ann. des Sc. Nat. Vol. ii. p. 135.

[§] British Reptiles, p. 92.

lity contained a large proportion of developmental cells that
ided spermatozoa not fully matured, but which might be-
e liberated in the water at a longer or shorter period. Or,
ibly, the fluid added to ova that had been long in the water,
been very recently obtained; in which case the vigorous
matozoa might effect the impregnation of ova that had be-
e almost insusceptible through the imbibition of water by
r envelopes. I am led to this view by the fact that the
-like envelope of the Frog's egg begins to imbibe and ex-
d the instant it is brought into contact with fluid; and from
ing ascertained that there is a close relation between the
ree of expansion and imbibition of this envelope and the
eptibility of the ovum to become impregnated, and that
se conditions are also greatly affected by temperature. The
of expansion of the envelope is an act of endosmose, and
sibly this is one of the means by which the impregnating
nt is made to exert its influence on the yelk. The yelk is
a passive recipient during the endosmic action of its cover-
s, but seems to participate in that action, as I have seen
tions of its surface heave and contract within the vitelline
mbrane during the first hour the egg has remained in water.
may thence be inferred, that if the impregnating stimulus be
supplied quickly, the fitness of the ovum to become im-
gnated is diminished in proportion as its envelopes are ex-
ded. If then it be proved that the spermatozoon is the
nt in impregnation, but, so far as can be discovered, does
t penetrate bodily into the ovum or its envelopes, and yet, as
y be shown, must always come into contact with their surface,
more rapidly and to the greater extent this expansion takes
ce, and removes the efficient body from that which it is in
ne way destined to affect, the less will be the chance of its
pregnating the ovum, and the less will the ovum become
ceptible of impregnation even by the most healthy and
ratile spermatozoa.

The extent and rate of expansion of the envelope of the
g's egg, during the first half hour it remains in water, very
arly coincide with the diminution of the fitness of the ovum to
come fecundated. This is shown by observing the rate of ex-
nsion of the envelope during the first fifteen minutes of sub-
rsion, and then testing the fitness of the ovum, by experiment,
ring a similar period.

At the *moment* when the ovum is expelled from the body, the
velope is merely a thin gelatinous layer, its entire diameter
ing equal only to about one sixth of the diameter of the yelk.
fter it has been *one minute* in water, and begun to imbibe and

expand, it is then equal to about one-fourth of the diameter of the yelk. At the end of *two minutes* it is enlarged to one-third, and in *three minutes* to one-half the diameter of this body.' In *four minutes* it exceeds three fifths, and in *six minutes* two-thirds, and it continues to imbibe fluid and expand at the same rate, until, at from *ten* to *fifteen* minutes, it very nearly equals in thickness the whole diameter of the yelk; and at *half an hour* it is one-fourth greater than this. PREVOST and DUMAS[*] noticed the expansion of the envelope during the first *six hours*, but entirely overlooked the rate of expansion during the most important period, the first hour, and noticed only the general fact that the diameter of the envelope, and the end of the first hour and a half, was as 5 to 2·5 at the time of spawning, and that it had nearly acquired its full size at the end of three hours. My own observations agree with this latter statement. The expansion of the envelope is greatly retarded at the end of the third or fourth hour, until after cleavage of the yelk has taken place, when it again proceeds, but much more slowly than at first. If then we bear in mind the rate of expansion of the envelope during the first half-hour, the following experiments will give some idea of the degree of susceptibility of the ovum to become impregnated during that period.

Mr Newport subsequently, on the 6th of April 1850, performed a series of six experiments, in which, after immersing in water, *ova* varying in number from eighty-three (83) to two hundred and five (205), and added impregnating seminal fluid, so as completely to cover the ova; and two experiments in which eighty-one ova, and one hundred and thirty nine ova (139), were exposed to dry air, and then subjected to the operation of seminal fluid applied in an effectual manner.

Of this series of experiments the following tabular summary indicates the results:—

TABLE I.—Set F.

Experiment.	Ova.	Time.	Medium.	Fluid obtained.	Segmentation.	Embryos.	Per centage.
		″		″	′ ″		
No. 1	83	1	Water.	43	3 55	32	·38
No. 2	92	2	Water.	43	3 55	45	·49
No. 3	127	3	Water.	17	4 3	33	·26
No. 4	81	3	Air.	8	4 5	53	·65·5
No. 5	136	5	Water.	20	4 8	10	·07
No. 6	139	5	Air.	12	4 11	37	·26
No. 7	205	15	Water.	26	4 14	45	·22
No. 8	100	30	Water.	44	......	2	·02

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[*] *Loc. cit.*, vol. ii. p. 108.

Thus, then, at a temperature of 60° Fahr., the susceptibility of the ovum to become impregnated is greatest at the time it is passed into water, and for two or three minutes afterwards, and segmentation then takes place more quickly, even when the seminal fluid has been for nearly three quarters of an hour mixed with water, than after longer immersion. The fitness of the ovum to become impregnated is gradually diminished, and segmentation takes place more tardily, according to the length of time which the ovum has remained in water, as is seen by comparing the results of Nos. 1 and 2 with 7 and 8. On the other hand, while the desiccating effect of exposure to air more arrests the fecundation of the ovum and the occurrence of segmentation of the yelk than a continuance for a corresponding length of time in water, it seems to be less prejudicial to the fecundity of the ovum than immersion in that fluid, as appears to be shown by comparison of Nos. 4 and 6 with 3 and 5, the difference in the number of ova produced being too great to lead us to attribute this to difference in the length of time the impregnating fluid had been obtained.

In the foregoing set of experiments, the quantity of impregnating fluid supplied to the ova was but little attended to, it being added very freely in each case. In the following set I was desirous of knowing what difference would result from the fluid being applied more sparingly, or but for a very short space of time. SPALLANZANI had made experiments with a similar view, but his appeared to be open to some objections, as he had not noted some important circumstances which greatly affect the result, as the temperature of the medium, the length of time the fluid employed had been obtained, &c. In the experiments now made, these circumstances were attended to, and I noticed a curious fact which I first remarked in experiments in 1849. It is what I may designate *partial impregnation*, and is indicated by a portion only of the yelk becoming segmented. This frequently happens with ova that have been brought into contact with only very small quantities of seminal fluid, and but for short spaces of time, as in some of the following experiments. These ova, so far as I have observed, never produce embryos. Segmentation is arrested in some at the very commencement; in others it goes on to the second or crucial fissure; and in a very few cases may proceed somewhat further; but it is never completed to granulation of the yelk. This, I think, is a fact which deserves some consideration with reference to the formation of the embryo.

[Another set of six experiments (F) had been previously instituted on the 22d of March 1850, in which *ova*, varying in number from forty-two to seventy-seven, were passed into water at 46°. 5, F., in one exposed merely to air, and in two experiments passed into a solution of carmine for five minutes and fifteen minutes respectively, and then impregnating seminal fluid, was applied to them in various modes.]

From these experiments, which were performed, in order to ascertain the influence of temperature, it seemed evident that the susceptibility of the ovum to become impregnated is diminished in proportion to the degree of expansion of its envelope and its imbibition of fluid, conditions which are greatly affected by the temperature of the medium in which the ovum is placed during the first hour; and there seems reason to suppose, that this diminution may be due to the extent to which the envelope becomes influenced by temperature, rather than to any insusceptibility at that time in the yelk itself.

[From these experiments, and other three performed on a female frog in which the opral chord had been divided four hours and a half previously, the following inferences are deduced.]

These facts proved that the ova employed were still fitted to become impregnated when the fluid was supplied to them in sufficient abundance, and for a sufficient length of time, and within the period during which the envelope continues to expand and imbibe most rapidly. This condition is always promoted by an early removal from a low to a comparatively high temperature during the period of expansion, as in *Set G*, in which segmentation took place in from *five hours and a half to six hours*, and when the removal from low to high temperature was within the first half hour; while it did not occur, in the only experiment in which it happened in *Set* F. No. 1, until the end of *six hours and a-half*, when the removal from a similar low to a like high temperature was not made until one hour and a-half after impregnation.

The influence of temperature is thus as marked on the impregnation of the ovum as it can be proved to be on the future development of the embryo. Impregnation is accelerated, and its occurrence also is more certain in a high than in a low temperature. In the latter it becomes retarded and is less determined. This applies equally to the susceptibility of the ovum, and to the fitness of the impregnating fluid to effect impregnation. But in proportion as this fitness is exalted by increase of temperature, so is the duration of the capability to receive in the one, and the efficiency to communicate in the other diminished. SPALLANZANI found that the ova of toads placed in an ice-house could be impregnated at the end of forty-one hours.* PREVOST and DUMAS† also mention that they were

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* Dissertations, &c., Vol. ii. p. 177.

† *Loc. cit.* Vol. ii. p. 140.

successful, and that too to a great extent, with ova that had been twenty-four hours *in water*, the temperature during the period ranging from 18° Cent. (64°. 4 FAHR.) to 22° Cent. (71°. 6 FAHR.), and with some eggs that had not been immersed even at thirty six hours, * the temperature being then from 12° Cent. to 15° Cent. (53° ·6 to 59 FAHR). The results obtained by myself have been much less successful. Out of one hundred and forty ova obtained from a female frog, killed twenty-four hours before and preserved at or below the temperature of 55° 5 FAHR., at which the experiment was made, only a very few became partially segmented, but not one produced an embryo; although an abundance of impregnating fluid, abounding with spermatozoa, and obtained only a few minutes before it was employed, had been supplied to them. It is evident, therefore, that this failure was due chiefly to the ova, and not to inefficiency of the impregnating fluid. On the other hand, I have been equally unsuccessful with ova from a frog that had been killed only two hours and a-half, when the impregnating fluid employed had been more than four hours and a-half mixed with water. In this case the failure appeared to have been due chiefly to the spermatozoa, nearly the whole of which, on inspection by the microscope, were found to be motionless and appeared to have lost their vitality. At the same time it must be mentioned, that the female from which the ova employed in No. 3 of the last set of experiments were obtained, still existed, in so far as the vitality of the muscular system was concerned, and therefore can hardly be mentioned in comparison with MM. PREVOST and DUMAS's observation.

But while the numerical results obtained by myself have been less favourable than those of SPALLANZANI or the physiologists now mentioned, the general facts, so far as they are open to comparison, are in full accordance with them. The difference in the details of our respective observations appears to have been due in chief part to the influence of temperature at the time of the impregnation of the ova, or within the first two or three hours after the impregnating fluid has been supplied. Thus, if the temperature has been gradually rising at the time of impregnation, the fecundation of the ovum, as I have stated, has more certainly taken place than when the temperature was subsiding, the condition of the ova and of the impregnating fluid employed being equally fit in each case. SPALLANZANI has shown that in his experiments ova did not become impregnated after they had remained fifteen minutes in water. In the experiments by myself, I could rarely obtain fecundation after thirty minutes' immersion. The difference of time between these results may fairly be attributed to difference in the temperature at which the experiments were made, and in great measure to the influence of this on the endosmosis and expansion of the envelopes.

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* *Loc cit.* p. 134.

But it was possible that some other agent might be concerned in these results, and that light, as well as heat and immersion in water, might greatly influence them. To put this to the test, and to learn whether the difference depends entirely or chiefly on the amount of temperature, I have made two sets of experiments at precisely the same time, performed in the same way, with ova from the same female, and impregnating fluid from the same male, the only difference being that within a very few minutes after the impregnating fluid was supplied, one set was removed to a higher and slightly rising temperature, from which all light was excluded, while the other was allowed to remain freely exposed to light, but in a room of ten or twelve degrees lower temperature, and which was becoming still further reduced.

The influence of light and heat on the development of the embryo has already been referred to by SPALLANZANI, PREVOST and DUMAS, RUSCONI, Dr W. EDWARDS, and Mr BELL. RUSCONI expressly states that light has no influence on the development of the germ,[*] but his observations, as well as those before made by SPALLANZANI, show that heat has a very marked influence, and this has been fully confirmed by Dr W. EDWARDS, and Professor BELL. Very recently also the subject has been referred to by Mr HIGGINBOTTOM,[†] and I have great pleasure in stating that my own observations on the influence of heat, and the little effect of light on the development of the tadpole, are in accordance with the observations made by him. But the object I have had most in view has been, as above stated, to mark the effect of heat, without light, on the changes of the ovum, more especially during the period of fecundation, the first three or four hours after the egg is laid, and onwards to the termination of what I shall hereafter propose to consider when describing the development of the embryo,—as the end of the *third period*—the closure of the *laminæ dorsales* and the establishment of ciliary aëration on the surface of the body.

[From two sets of six experiments of the same nature performed on the 20th March 1850, the conclusions deduced are the following.]

Thus while segmentation took place in No. 1 H, in three hours and fifty-six minutes, when the temperature was rising

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[*] *Loc. cit.*. p. 20.

[†] Proceedings of the Royal Society, May 16, 1850, and Phil. Trans., Part I, 1850. See also Ed. Med. Journ., lxxvi., 493, Oct. 1851.

from 59° FAHR. to 64° FAHR., it did not occur in No. 1 I, until seven hours and forty-five minutes, when the temperature during the interval was sinking from 48° FAHR. to 47° FAHR. This sufficiently marks the great influence of temperature during the earliest periods of change in the ovum; and this injurious effect of reduction of temperature at that period is further shown in the relative number of embryos in these comparative experiments. That the injurious effect of reduced temperature at the time of impregnation is mainly the cause of this result, and not the diminution of temperature after the period of impregnation, seems to be shown in the circumstance, that while at the end of the eighteenth hour the ova in the set H had already passed through all the stages of segmentation, and the surface of the yelk had become granulated, and the blastoderma had begun to be formed even although the temperature in that case subsided a little after segmentation had commenced,—from 59° to 57°,—the corresponding set of ova, No. 1 I, had advanced only to the octuple division of the yelk. A similar difference in the rate of development we have seen takes place in the growth of the embryo. At the end of three days the embryos of set H were advanced to the stage at which the laminæ dorsales are proceeding rapidly to meet, and form the median dorsal sulcus of the growing body. But the corresponding ova in set I, had not been carried further than to the earliest perceptible indications of the *area germinativa.*

These facts sufficiently prove the great influence of temperature on the development of the embryo in its earliest stages, as the comparative numerical results do also its effects on the impregnation of the ovum. Out of eighteen ova placed in the higher and increasing temperature, thirteen produced embryos at nearly similar stages of growth; while of nineteen ova maintained in a low and diminishing temperature only eight became segmented, and but three of them arrived at the tadpole state.

A somewhat similar but more marked result took place with the ova of No. 2 in the two sets of experiments. The impregnating means employed in these trials had already been forty minutes mixed with water on the filter. Out of forty-eight ova employed in set H, twenty-five became segmented, and ten of these produced young. But in set I fifty-one ova gave birth to only two embryos.

In the third experiment of each set the difference is as strongly marked. As the filtered fluid employed in both was the same, and the very few spermatozoa contained in it were by the same means brought into contact with the ova in each, it might have been expected that each would have produced embryos. But while the production of a single tadpole in the one case, at a high temperature, may be looked upon as leading to the inference that these bodies are the efficient agents in impregnation, the entire absence of all appearance of impregnation in the ova of the other set, to which the same fluid had been equally applied, seems to point to the cause of failure in this case as depending on the prejudicial effect of a low temperature of the surrounding medium on their agency.

The temperature of the surrounding medium ought, therefore, always to be borne in mind when we are attempting to deduce conclusions from experiment on impregnation and development. The presence of light appears to be only of secondary consideration as compared with heat; since in set H, from which light was carefully excluded, not only did impregnation take place more certainly and rapidly than in set I, which were exposed to light, but the embryos also were produced in greater number, and acquired maturity in less than one-half the space of time than in the latter; the only difference of circumstance between the two sets being degree of temperature.

Influence of Aëration.—Next in importance to heat is a free *aëration* of the ovum. This is of less consequence with reference to impregnation than to the subsequent production of the embryo. In every set of experiments there are always some ova more advanced than others. These are ova which have been nearest to the surface of the water, and which, consequently, have been more completely aërated as well as exposed to a slightly higher temperature than others at a greater depth. It is from this cause chiefly that the results of experiments on artificial impregnation, and even of observations on naturally impregnated ova, are always less complete and successful than what takes place with regard to the ova in the natural haunts of the species. The ova in a state of nature are usually deposited in well-aërated places, clear, slow-moving water, or shallow and but slightly turbid water. It is almost impossible to afford to ova that are the subjects of experiment, either in broad flat dishes, or in glass vessels in one's study, the amount of aëration required to ensure complete success. By too frequently changing the water in the vessels the embryos often become injured; while if the water be not changed, development is arrested, and decomposition commences, and the experiment entirely fails. Even when these difficulties are obviated by a gentle withdrawal of the water, and a renewal of it with equal care, the perfect stillness of the fluid in the interval of our observations does not allow of that extent of aëration to the embryo which it gains in a perfectly natural state, either in slow-moving waters, where I have usually found the eggs deposited, or in pools of still water, the surface of which is agitated by currents of air, and affected by diurnal changes of temperature.

Thus, then, we may conclude that the procreative force of the germ, and of the impregnating fluid, is augmented by increase of heat, but the duration of the force is lessened. It becomes less and less energetic in proportion as the temperature is diminished, but the period during which it is capable of being exerted is extended. In each of these conditions aëration is of essential consequence, and becomes more and more necessary in proportion to the increase of heat.

(*To be concluded in next Number.*)

ART. VII.—*Account of a Case in which the Cæsarean Section was performed; with Remarks on the Peculiar Sources of Danger attendant on the Operation.* By CHARLES WEST, M.D., Physician Accoucheur to St Bartholomew's Hospital, and Lecturer on Midwifery in the Medical College.—(Received December 20th, 1850.—Read January 28th, 1851.) (From the Med. Chir. Trans., Vol. xxxiv. 1851.)

ELIZABETH WILLIAMS was one of eight children, of whom seven reached adult age. Her father, who had been insane for some years previously, died at the age of 73, her mother at that of 68. Two of her brothers and one sister died of phthisis, and this sister was insane for some years before her death. Two brothers and a sister survive in good bodily health, but the sister is of weak intellect: and Elizabeth herself had, on more than one occasion, shown indications of insanity.

As a child, E. Williams' health had been good, but it became much impaired about the age of 15, when she began to menstruate; and ever since then she had frequently, indeed almost constantly, suffered from pain in the back, referred especially to the pelvis. About the age of 23 the pain became much severer than it had ever been before; but, in spite of this, and of the advice which she received from some medical man not to marry, she married, at the age of 25, the husband of her deceased sister. She did not become pregnant until fifteen months after marriage; but conception was followed by a very considerable increase of pain, associated, during the latter half of her pregnancy, with a great and rapidly increasing difficulty in walking. For some weeks before her confinement she did not move further than from her bedroom on the second floor to her sitting-room on the first floor, where she generally spent the whole day lying or sitting upon her sofa. Sometimes, however, she moved about her room, and on the day before her confinement, was occupied in putting up curtains to the windows; her disinclination to walk seemed to be due, at least, as much to her nervous temperament and wayward disposition, as to any physical incapacity for exertion.

She was in the fifth month of her pregnancy when she placed herself under the care of Mr Wren, of Brownlow Street, to whose kindness I am indebted for many facts in her history, as well as for the permission to lay her case before this Society. There was nothing in her appearance, either when she first came under Mr Wren's care or subsequently, to suggest the idea of her being deformed; the only peculiarity which she presented being that she always stooped very much forwards. During her pregnancy her bowels required the constant use of purgatives; her appetite was bad, she was much annoyed by heartburn, had occasional faintings, and suffered much from palpitation; her pulse beating habitually 120 in the minute, (a rate of frequency which she said it had manifested for years), but varying under every source of excitement. She was nervous and excitable in the highest degree, looking forward with much apprehension to her labour, and especially to the pain attending it; and was urgent in extorting a promise that she should be permitted to inhale chloroform when labour came on.

It was in this unfavourable state, both of body and mind, though much benefited by Mr Wren's treatment, that Mrs Williams reached the end of her pregnancy, and labour pains came on at 1 A. M. on the 7th of May 1850; she having been married just two years, and being in the 27th year of her age.

Mr Wren was summoned at 3 A. M., and discovering, on his first examination, the existence of extreme pelvic deformity, despatched a messenger for me, and I arrived at a quarter to 5 A. M. I learned that the pains which the patient had had were very feeble, and returned only about every quarter of an hour. She bore them, however, very ill, tossing about the bed, crying out for chloroform, and becoming almost unmanageable on any attempt to make a vaginal examination.

She was a small, slender person, and, as she lay in bed, presented no sign of deformity, and her extremities were straight and well formed. Her face was pale, and her pulse frequent and feeble. On examining her spine, it was found to be perfectly straight, but the sacrum was bent into an almost semicircular form, with a great convexity projecting backwards; the pubic arch was a little wider than would suffice to allow of laying one finger between the rami of the pubes; the tuberosities of the ischia did not seem to be much above one inch apart, and the rami of the pubes ran out into a sort of beak; the bones being bent at the junction of the pubis and ischium, and being at this point not more than one inch apart.

The *os uteri* could be reached with difficulty; it was open to about the size of half a crown; its lips were still thick, but soft.

At a quarter to 10 A. M., Dr Ramsbotham and Dr Murphy saw the patient with Mr Wren and myself; her case appearing to us to be one in which, even irrespective of the interests of the child, the Cæsarean section was called for. Auscultation, moreover, had ascertained the child to be living.

Pains had occurred, though feebly, since 5 A. M, and the *os uteri* was now found by Dr Ramsbotham to be fully dilated. During his examination, the membranes broke, and *liquor amnii* escaped, though not in any large quantity. The head was found to be presenting.

The circumstances which seemed to point out the Cæsarean section as the only admissible proceeding, were not merely the probability of the existence of a very considerable degree of contraction of the pelvic brim, but also the extreme narrowness of the outlet, which would render the introduction of the hand to guard the embryotomy instrument impossible; while even supposing the head of the child to have been so mutilated as to admit of its extraction, it was yet doubtful whether the body could be extracted, and the rather, since the narrowness of the outlet would quite prevent any attempt at evisceration from succeeding.

On the other hand, it might be alleged, that though any attempt at extracting the child would be both dangerous, difficult, and tedious, and would most likely occupy some two or three hours, yet it could probably be effected. Although it was quite true, that, should serious difficulty occur in extracting the body, it would not be possible to eviscerate the child, yet the chances were against the existence of any such difficulty as to be absolutely insurmountable; while, though injury might be inflicted on the mother, yet injuries to parts within the pelvis being so much less formidable than injuries to the uterus itself, the endeavours to deliver by the natural passages would, on the whole, be preferable, as far as the mother was concerned, to the more hazardous experiment of the Cæsarean section.

Dr Ramsbotham's opinion inclined to the latter view; but although he thought the fœtus might, by great exertion, be dragged through the pelvis after craniotomy had been effected, he nevertheless acquiesced in the view that Dr Murphy and myself took of the great danger necessarily attending such forcible efforts; and this, in addition to the possible chance of ultimate failure, induced him to agree with us, and to sanction the performance of the Cæsarean section. It was determined to request Mr Skey, who was the only surgeon in the metropolis who had ever performed the operation on the living subject, to undertake it. To this request Mr Skey kindly acceded, after he had been made acquainted with the particulars of the case; and it was in conjunction with him that the subsequent treatment of the patient was carried out.

As no circumstance seemed to indicate the necessity for earlier interference, 4 o'clock, P. M., was the hour fixed for the operation. For permission to lay before the society the details of its performance, I have to acknowledge my obligation to the kindness of Mr Skey.

On assembling at 4 o'clock, we found that the patient had had but very little uterine action; her bowels had been relieved by an enema, and she had taken a little beef-tea occasionally. The necessity for the performance of the Cæsarean section being explained to her, she readily consented to undergo it, but on condition that she should be previously rendered insensible to pain by the use of chloroform; and no representation of the comparatively slight suffering which the operation would occasion, or of the possible increase of danger to which the employment of chloroform might expose her, could at all shake this resolution.

On receiving the assurance that her desire, in this respect, should be complied with, she at once became contented, went quietly through all the preliminaries of the operation, and, during her subsequent illness, showed a remarkable degree of equanimity and patience, which were interrupted only when the wound was dressed, and the apprehension of pain then rendered her immediately excited and difficult to manage.

The patient was placed on a table, with her feet resting on two chairs, and her shoulders somewhat raised; the catheter was passed, and a small quantity of urine drawn off; the fœtal heart was distinctly heard beating in the right half of the abdomen, but the uterine souffle was not clearly perceived anywhere; facts, the inferences from which were confirmed during the operation.

An assistant being stationed on each side of the patient, to prevent the escape of the intestines, and the inhalation of chloroform having begun, Mr Skey marked the line of the incision with ink; and so completely was the contour of the abdomen altered by the contraction of the pelvis, that the incision, seven inches in length, was carried two inches above the umbilicus, and down to the commencement of the pubic hair, and it was afterwards found necessary to carry it an inch higher.

Inhalation of chloroform began at 18 minutes past 5; at 25½ minutes past 5 the first incision was made, and at 31½ minutes past 5 the child was extracted, the operation having been somewhat delayed by the circumstance that the patient began to struggle after the first incision, so that it became necessary to pause, in order to give her more chloroform.

The abdominal teguments were extremely thin; no bleeding took place from them. On opening the peritoneum, about a drachm of transparent serum escaped; and on dividing it to the whole extent of the external wound, the uterus presented itself, occupying it so completely that no intestines were to be seen. The uterus was turned obliquely on its long axis, the right Fallopian tube and ovary being distinctly in view, the front of the uterus looking towards the left side, and the organ corresponding in its direction very nearly to that of the left oblique diameter of the pelvis. Care was taken, however, to make the incision in the mesial line of the uterus. The walls of the outer half of the uterus bled comparatively little; but the bleeding from the vessels of the inner half was free, though not alarming. The total thickness of the uterine wall was about three quarters of an inch. The cavity of the uterus was opened at the lower part of the wound, and a director of peculiar construction, invented by Mr Skey for the purpose, and used by him in his former operation, was then introduced, and the incision carried upon it quite up to the fundus of the womb. The child was now seen lying in the third position of Naegele (the second of most authors), namely, with its back to the right, and forwards. The membranes were at once torn through, and the child, a fine girl, was taken out. Its surface looked slightly livid, and for a few seconds it did not cry. It then, however, began to breathe, and in two or three minutes cried loudly, without any measures having been needed to resuscitate it.

It may, perhaps, be as well to add, that the child still survives, and is in good health.

The uterus did not contract much. It diminished, indeed, in length; but the wound remained widely open, and the placenta was seen attached to the posterior part of the organ. The uterine cavity was now filled with blood, and the placenta, looking as if it were entirely detached, it was removed by Mr Skey; who found, however, that its complete separation required rather firm traction.

No sooner was the placenta removed from the uterus, than the blood welled up from it, partly from its interior, partly from its cut edges, just like water gushing from a spring; a profuse, steady, uniform stream running down upon the floor, and which seemed, for a few seconds, as though it would not cease so long as there was any blood to flow. From a pint and a half to a quart of blood was thus lost; and, during the continuance of this very profuse bleeding, the patient's pulse became very feeble, though it never altogether ceased; while, owing to the insensibility previously produced by the chloroform, it was some minutes before she could attempt to swallow. The first indication of returning consciousness was furnished by several violent attempts to vomit, during which the intestines (especially on the left side of the uterus), which had come into view immediately on the removal of the child, prolapsed, and could with difficulty be retained in the abdomen, even by closing the integuments over it.

By degrees the hæmorrhage abated; no other means having been resorted to, to excite uterine action, than moderate pressure with the hand on the abdomen. At 6 p.m., the hæmorrhage had quite ceased, though the uterus was not firmly contracted, nor the wound closed tight. Five sutures were now placed in the abdominal integuments, the last, one inch and a half above the lower edge of the wound; a space being left here at my request, in compliance with the suggestion of some continental surgeons, who attach importance to an aperture being left for the escape of the discharges from the uterine wound. Broad strips of plaster were next applied; and an eighteen-tailed bandage, which had been placed under the patient before the operation was begun, was now brought together over some large sheets of cotton wool, which served to protect the abdomen from pressure.

The patient was now carefully lifted into bed; and immediately on being placed there, efforts at vomiting came on, during which firm pressure was made on her abdomen, to prevent, if possible, protrusion of the intestines. As soon as they had a little subsided, sixty drops of tincture of opium were given in a little brandy, but were almost immediately rejected, and she refused to take any more laudanum, saying it would make her vomit.

At this time, though her pulse was extremely feeble and her extremities were cold, yet her mind was calm and collected; nor did her intellect become at all disturbed, until within an hour or two of her death.

Two doses of morphia were given her between 7 and 10 p.m., but both were rejected soon afterwards, and the patient made frequent efforts to vomit during the first two hours after the operation. At 10 p.m., however, she expressed herself as feeling comfortable, and her pulse, though extremely feeble, did not exceed 108 beats in the minute. On examining the wound, a portion of omentum was seen to have escaped beneath the lowest suture, and having been returned with some difficulty, an additional suture was put in to close the lower edge of the wound, and the renewal of the accident was thus prevented.

In consequence of the irritability of her stomach, it was determined to endeavour to keep the patient under the influence of opium, and to supply her with nourishment mainly by means of enemata, and, accordingly, a drachm and a half of laudanum was thrown into the rectum at midnight, and was followed in about an hour by an enema of four ounces of strong beef-tea thickened with isinglass. The laudanum in smaller doses and the beef-tea were given in enemata at intervals of about every four hours; so that during the first sixty-two hours after the operation, five drachms fourteen minims of the former, and forty-eight ounces of the latter, were thrown into the rectum; the beef-tea being thickened with isinglass, and occasionally mixed with brandy.

From the time of the operation to her death, the patient was watched incessantly by Mr Cupiss, Mr W. H. Stretton, Mr Arthur Stretton, or Mr Hillier, four very intelligent students at St Bartholomew's, who not only did everything which the most unwearied care could do to save her, but also recorded, with the minutest accuracy, every change in her condition.

For the first twenty-four hours she dozed at intervals, though she continued in a state of great depression and vomited occasionally; but the irritability of the stomach gradually diminished; the lochial discharge appeared, and continued to flow scantily, but without any unnatural character during the remainder of the patient's life. She passed water, throughout the whole of her illness, without any difficulty, and suffered scarcely any pain; while the distressing efforts to vomit were always relieved, sometimes entirely arrested, by firm pressure upon the abdomen. Her condition during the 8th of May continued one of great depression; and though her sickness was less, yet she took nothing more than a little ice and a spoonful of milk occasionally. Her pulse was 148 at 10 P. M., having been 160 at the same time on the previous day. Moreover, she bore the cutting off some of her soiled linen very well, and expressed a wish for some wine.

She passed the night of the 8th of May on the whole comfortably, dozed a good deal, and took wine and water frequently, but was slightly sick at 5 A. M. on the 9th, when she threw up a little clear fluid, being the first time of her vomiting for more than twenty hours.

During the day she took sago and milk readily, but refused wine, and continued apparently better till half past 3 P. M., when she vomited a dark brown offensive fluid; and this vomiting recurring thrice before 10 P. M., left her evidently deteriorated, and she passed a more restless night than the preceding one had been.

At a quarter to 7 A. M., on the 10th, vomiting of green matter had returned, and the patient seemed more feeble, She had taken very little by the mouth, and the disposition to sickness and distaste for almost everything, with the inability to bear more than a teaspoonful or two of any nourishment at a time, interfered greatly with all endeavours to support her powers. She had complained sometimes of headache, especially when sickness was felt; and it became a question with Mr Skey and myself whether any advantage likely to accrue from the continuance of the laudanum in the enemata was such as to counterbalance the possible evil of maintaining a constipated condition of the bowels, and of keeping up or aggravating the patient's sickness. The character of the matters vomited seemed, indeed, to point to a more serious and less remediable cause; but still it was determined to try the effect of omitting the laudanum.

Various stimulants and various articles of food were tried during the course of the day, but she took very little of any; champagne and ice being the two things that seemed most grateful to her. She had one or two returns of green vomiting during the day, but did not appear, at 11 P. M., to have at all retrograded since the morning.

She became very restless, however, and was sick twice after 11 P. M.; the opium was accordingly resumed in the enemata, and early on the morning of May the 11th, small doses of the black drop were given every two hours at Dr Murphy's suggestion. By 3 P. M., under the continuance of the opium and the enemata of beef-tea, with such small amounts of food and stimulants as she could be induced to take, she rallied surprisingly; the lochial discharge, which had almost ceased for the previous twelve hours, reappeared; and her pulse, whichhad ranged at about 140 to 150, though still 146, was somewhat less feeble.

At 3 P. M., Dr Ramsbotham, Dr Murphy, Mr Skey, Mr Wren, and I, saw her together, and felt more hopeful about her than at any time for the previous twenty-four hours. There was, however, one bad symptom about her even then, which was, that her temperature, which during the night had sunk very low, continued so, and that there was a cold perspiration upon her surface. Almost immediately after this visit, too, the other favourable symptoms began to disappear; her skin became quite cold, her pulse thready, and she sank into a state of collapse like that of a cholera patient, her intellect continuing clear, and considerable muscular power remaining, until within a couple of hours of her death, which took place at half past 6 A. M., on May 12th, 108½ hours after the operation.

At the examination of the body 33½ hours after death, the abdominal cavity and the pelvis were alone examined.

Decomposition had already advanced; the body gave out a very offensive odour; there were livid spots on the surface of the abdomen, which was tympanitic.

The external wound was now six inches long; union had taken place for about two inches at its lower edge, and about the situation of the fourth suture, but elsewhere its edges were gaping.

A portion of healthy omentum, about two inches long, was found lying free at the lower edge of the wound beneath the integuments, but external to the peritoneal cavity.

There was no effusion of fluid into the abdominal cavity; the viscera were all exceedingly bloodless, but a thin layer of dirty yellow lymph connected the opposite surfaces of the pale, large intestines to each other, and also at some points united the intestines to the uterus.

A thick layer of yellow lymph lined the abdominal walls from the umbilicus downwards. On stripping it off, the peritoneum appeared thickened, and destitute of its natural polish, but scarcely, if at all, increased in vascularity. The intestines contained very little fluid, no solid feces, some flatus.

The bladder was empty and flattened, and appeared to have adapted itself to the altered shape and relations of the parts, being almost completely out of the pelvis. It was quite pale and healthy.

The uterus was moderately contracted, but not the slightest effort had been made to close the wound, the external edges of which were retracted, and lay at least one inch apart, while the internal edges were but just in apposition.

At the wound, the substance of the uterus looked swollen and infiltrated, the wall there measuring 1·2 inch in thickness, while at the fundus of the organ it was only ·7 thick. The length of the external uterine wound was 3·65 inches; of the inner 2·5. The edges of the wound were covered by a thin layer of a dirty brownish matter, in all probability altered blood; beneath which they were of a pale dirty, straw colour. Elsewhere the substance of the organ was pale and bloodless, looking as white as veal; presenting no sign of inflammation, no thickening of its veins, neither coagula nor blood in their channels.

The placenta had been attached posteriorly near the fundus of the uterus, but rather to its left side. The surface to which it had been attached looked quite healthy, as did the whole interior of the uterus, and also the vagina, in neither of which was there any blood nor any sort of morbid secretion.

The pelvis was a very well-marked specimen of the deformity produced by mollities ossium.

The lumbar vertebræ had been driven down into the pelvic cavity, so that, as the body lay upon its back, a line drawn from the symphysis pubis directly backwards, touched the upper part of the fourth lumbar vertebra.

The length of such a line was 4·2 inches.

Transverse diameter of the brim 4· „

The rami of the pubes were projected forwards into a beak, the width of which was ·75 of an inch, while its length, measured from the inner surface of the symphysis, was 1·2 inch; by which the antero-posterior diameter was reduced to 3 inches.

The bodies of the fourth and fifth lumbar vertebrae were much flattened, so that from the upper part of the fourth to the upper part of the sacrum they measured only 1·5 inch.

The sacrum was so greatly curved that from about its third vertebra it ran horizontally forwards. The depth of the bone to the point where it bent forwards, was rather less than one inch and a half; while from the point of the coccyx along the horizontal part of the sacrum it was 2·75.

Distance between the two anterior superior spines of ilia ...	8 inches.
... . tuberosities of ischia, ...	1·2 ...
... ... point of coccyx, and summit of the pubic arch	2·8 ...
... .. rami of the pubes at the lower edge of the beak	1·05 ...
Width at the upper part of the pubic arch	·6 ...

Agreeing, as I do, most cordially with the rule laid down in British Midwifery, which gives the mother's life a claim paramount to every other consideration, it cannot but be with a feeling of deep regret that I am compelled to add another to the already long list of failures of this operation.

I am acquainted with 409 authentic cases of the Cæsarean section, 341 of which are collected in Kayser's very valuable essay De Eventu Sectionis Cæsareæ. In 251 of these the mother died; in 158 she survived. The fate of the children is mentioned in 347 instances: in 110 of which they were stillborn, in 237 they were born living. There can, however, be no doubt but that these figures convey a very exaggerated impression as to the proportion of recoveries, and that the unfavourable estimate of English authors is nearer the truth. Both Kayser and Naegele regard the results given by the published cases as unfair; and the former mentions the fact, which of itself affords strong evidence on this point, that while the total maternal mortality among the cases which he had collected was 63 per cent., the mortality of cases occurring in lying-in hospitals, in which institutions failures must, of necessity, be reported as well as successes, amounted to 79 per cent. This last figure does not differ very materially from that afforded by the cases occurring in this country; 49 of which yield 7 recoveries, (or 8, if Mr Whitehead's case, where the patient survived till the thirty-second day, and died then chiefly from the advance of disease in her hip, be classed among the number;) the rate of maternal mortality being in the former case 85·7, in the latter 83 6 per cent.

Happily, however, in this instance, the painful question which sometimes besets us after an unfortunate operation, as to whether some different course might not have been adopted with better prospects of success, does not arise; since the extreme degree of pelvic deformity must have rendered any attempt at extracting a mutilated child almost certainly unsuccessful; while the absence during pregnancy of evidence of the contracted state of the pelvis sufficiently accounts for no measures having been adopted for the induction of premature labour.

But though there was here no alternative left, and though if a similar case presented itself to me to-morrow, I should again advise the same course, yet it cannot be denied that the Cæsarean section is attended by perils peculiar to itself, such as beset no other operation either in surgery or obstetric practice, and which, I confess, would make me shrink from recommending its performance in every instance where the way seemed open for any other proceeding.

The dangers peculiar to this operation, and inseparable from it even when most carefully performed, may be referred to the following four heads:—

1. The danger arising from hæmorrhage, which proceeds from a source different from that whence bleeding takes place in any other operation, and which is not capable of being arrested by the same means as suppress it under ordinary circumstances.

2. That dependent on the shock inflicted on the nervous system, as well by the violent interference with the most important process that ever goes on in the organism within the same limited time, as by the injury to a part so important and so richly supplied with nerves as the uterus of a parturient woman.

3. The hazard inseparable from extensive injury to the peritoneum, when unblunted in its sympathies and unaltered in its texture, as in cases of ovarian or other tumours, for the removal of which a similar exposure of the abdominal cavity is sometimes practised.

4. That which results from the infliction of a wound on the uterus, at a time when, in the ordinary course of things, the

processes which nature is prepared to carry on in it, consist in the desintegration and removal of its tissue; the very opposite, indeed, to those essential for the repair of injury.

To all, except the last of these sources of danger, attention hasbeen more or less directed; but still the amount of peril to which the patient is exposed from each, has, perhaps, been scarcely sufficiently investigated.

Kayser, in his valuable essay on the Cæsarean Section, states the cause of death in 123 cases; in 77 of which the patient died of inflammation, in 30 from the shock to the nervous system, in 12 from hæmorrhage, and in 4 from some accidental occurrence not of necessity associated with the operation. In some of these cases, however, no post-mortem examination was made; in others the date of the patient's death is not stated; while in several, more than one of the above-mentioned causes had contributed to occasion the patient's death. Selecting, therefore, such of Kayser's cases as were complete in all respects, and adding to them such others as have since come to my knowledge, with the requisite details, I have drawn up the accompanying table as affording some approximation to a correct view of the causes of death after the Cæsarean section.

From this table* it appears that, in 41 out of 174 cases, a notable amount of hæmorrhage occurred either during the operation or subsequent to it; and in a third of this number hæmorrhage was the sole cause of the patient's death. In seven instances the hæmorrhage arose, in great measure, from the placenta being wounded in the course of the operation; an accident which probably might be avoided in the majority of instances, by careful preliminary auscultation. In twenty instances it occurred at the time of the operation, and proceeded in part from the edges of the wound, in part, and usually in greater measure, from the seat of the placenta, and followed its detachment or spontaneous separation. In the remaining fourteen cases, the more important bleeding was secondary, taking place after the completion of the operation and closure of the wound, escaping externally in one or two instances, but in the other cases being poured out into the abdominal cavity, and being discovered in the form of coagula of greater or less magnitude, on examination of the patient's body after death.

Against this hæmorrhage, the resources of art can effect but little. The injury of the uterine substance can hardly fail to impair the contractile power of the organ, while that arrangement of the uterine sinuses which tends to prevent the occurrence of hæmorrhage after the separation of the placenta, even where the womb remains uncontracted, can be of no service in checking the bleeding from the wound.

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* *Vide*, p. 403.

It does not seem easy to say why profuse bleeding has occurred in some cases and not in others; and the impossibility of estimating the dangers of the operation before-hand with any near approach to correctness, is to my mind by no means the least of the difficulties that attend upon it. It may, perhaps, be thought that, in the case related above, the administration of chloroform had something to do with the very tardy contractions of the uterus; and my own experience of this agent, which I frequently employ in obstetric operations, inclines me to believe that this may have been the case, though it must not be forgotten, that the uterine action was very feeble even from the commencement of labour. Serious hæmorrhage attended on four* out of seven cases of the Cæsarean section, in which anæsthetic agents were employed; but in two,† the action of the uterus continued vigorous, although the patient was in a state of complete unconsciousness; while Mr Skey's patient, on whom he operated at St Bartholomew's, was never thoroughly under the influence of the ether; and the hæmorrhage in Dr Nimmo's recent case depended, in great measure, upon the placenta having been wounded. The risk of serious hæmorrhage is doubtless diminished by not operating till after the *liquor amnii* has escaped or been evacuated; but the danger of exciting violent peritoneal inflammation is probably a sufficient reason against following the practice of Professor von Ritgen of Giessen,‡ who advises that the uterus be drawn forwards, and surrounded with sponges dipped in cold water, till vigorous and permanent contraction has been induced; though in one or two cases, which terminated successfully, this proceeding has been adopted.

The shock to the nervous system is a very serious cause of the mortality of patients who undergo the Cæsarean section. In 33 of 147 fatal cases, the patient sank under the shock; no hæmorrhage having either attended the operation or followed it, and no morbid appearances having been discovered after death. In eleven cases more, though the more prominent symptoms during life were those of shock, yet examination after death discovered the effects of previous inflammation, and reference has already been made to nine other cases wherethe symptoms of shock might be, in measure, due to the loss of blood.

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* Namely, the present case, and those of Mr Skey, Dr Nimmo, and Professor Hüter ; numbers 109, 111, 102, and 8 in the Table of reference.

† The cases of Mr Campbell, No. 60, and Dr Oldham, No. 131 ; besides which it was employed with advantage in Dr Oldham's late successful case, reported in the Medical Times of August 16, 1851.

‡ Neue Zeitschrift für Geburtskunde, Bd. ix., s. 212.

Two causes, both alike beyond the control of the practitioner, expose the patient to this shock: one, the sudden interruption of the labour; the other, the injury to the womb; and the feeble person, exhausted by previous disease and suffering, and therefore least able to bear up against their influence, is she whose case, in general, most urgently calls for the performance of this operation. The employment of opium in large and frequently repeated doses, as originally suggested by Dr Stokes, in cases of peritonitis from perforation of the intestines, and of the successful adoption of which, in a case of rupture of the uterus, Dr Mitchell has related* a very interesting example, would probably do something towards warding off the symptoms of collapse. The discontinuance of the opium, in the case just recorded, was probably injudicious; and the patient's brief rallying afterwards was, perhaps, due to its re-administration. At the same time, the recovery of those patients who have survived the operation, does not seem to be attributable to the adoption of any one appropriate and well-considered plan of treatment; but their history is characterised by the very slight degree in which the shock was experienced, or the rapidity with which it spontaneously passed away; so that the fortunate issue of the operation appears to have been due to the vigour of the patient rather than to the skill of the doctor.

In fifty-six out of the 147 cases, or in 37 per cent., the symptoms during life, and the appearances discovered after death, were those of inflammation of the peritoneum, or, in some cases, of the uterus, and, in not a few instances, the morbid processes are said to have issued in gangrene. In twenty-nine cases more, the consequences of inflammation were found after death, though its symptoms were more or less masked by those dependent on previous hemorrhage, or on the shock of the operation. It certainly can be no cause of surprise, that in eighty five out of 147 cases, or in 57 per cent., the evidences of peritoneal inflammation should have been present; for, in addition to all those influences peculiar to child-birth, which render peritoneal inflammation more frequent then than at any other time, there is superadded the extensive injury inflicted by the operation, the long exposure of the abdominal cavity, and the subsequent escape, in many instances, of blood, or of the uterine discharges into it. Moreover, in addition to all of these causes tending to produce serious inflammation of the peritoneum, there is the absolute necessity of the occurrence of some degree of inflammation for the closure of the wound, and the repair, in as far as nature can effect it, of the grievous injury which has been inflicted by the operation. How narrow must be the limits which, in a patient who has undergone the Cæsarean section, separate the healthy action essential to repair, from the morbid action that tends to destruction!

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* Dublin Journal of Medical Science, Jan. 1843.

The state of the wound has, unfortunately, engaged less attention than the interest and importance of the inquiries on which it bears, might well demand. Still, even from the imperfect data with which we are furnished, it seems clear that one of the first steps which nature takes towards the repair of the injury in these cases, consists almost invariably in the establishment of inflammation of the peritoneal surface of the uterus, and of the corresponding portion of the abdominal peritoneum, or, less often, of the peritoneal surface of the intestines, with the view of forming adhesions all round the wound of the uterus, and thus isolating it from the cavity of the abdomen. This being effected, adhesion takes place between the cut edges of the peritoneum, which, lymph effused upon its surface, strengthens, though this often takes place but slowly, and, for a long time, imperfectly; as, for instance, in a case related by Professor von Ritgen,* in which a fistulous communication between the surface of the body and the interior of the womb did not close till the sixty-fifth day. Afterwards the divided tissue of the uterus itself, in some instances, becomes united, though this last step always takes place most imperfectly, and sometimes not at all.

In many of the fatal cases it has been found that no step whatever has been taken towards repair; in more the process set up has been a perverted one, and there is no attempt at adhesion; but a dirty exudation covers the pale and bloodless peritoneum extensively, or is poured out in a fluid form into the abdominal cavity; while in others, as already mentioned, parts are found with their vitality destroyed, and this not by the excessive activity of powers which, moderated, might have conduced to repair, but by their deficiency.

And this brings me to the last cause of the high mortality which follows this operation, and a cause against which skill can avail absolutely nothing, since it is inseparable from those processes which nature establishes after the uterus is emptied of its contents, be the period of pregnancy, at which that takes place, what it may. In a large proportion of cases, the record of the examination after death states, that the wound of the uterus was found gaping widely, even many days after the operation was performed. In other instances it is stated that the inner edges of the uterine wound were in contact, but the outer were far apart; and that, along the whole wounded surface, no indication was to be found of any attempt at its closure; while I know of but two instances in which the edges of the uterine wound are said to have presented a granulating surface.[*]

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* Neu Zeitschrift für Geburtskunde, Bd. ix. Heft ii.

Tardy, however, though the union of the wound of the uterus is, it yet takes place eventually, if the patient survives; and the history of the subsequent pregnancies and labours of women who have undergone the Cæsarean section, proves that this union must, in some cases, have acquired a considerable degree of strength. The great majority of instances, however, in which women have survived the operation, illustrate, in spite of the incompleteness of their record, the serious difficulties in the way of a cure, and the imperfect manner in which it is almost invariably accomplished. The giving way of the uterus at the cicatrix of the operation in a subsequent pregnancy, and the escape of the ovum into the abdominal cavity, are occurrences with which all who are conversant with the history of the Cæsarean section are familiar.[†] Imperfect, too, as the accounts of the cicatrix are, in those cases in which death has occurred after the lapse of some months from the performance of the operation, they yet suffice to show the frail nature of this bond of union. The womb, indeed, is generally adherent to the abdominal walls, but its divided substance is almost always found ununited; the edges of the wound being closed merely by the union of the peritoneum. Even where the union has gone deeper, it has yet been effected only by means of a dense, quite unyielding, or but slightly elastic, cellular tissue; in some instances, of such low organisation as to have been found converted into a hard, almost bony, substance. How slow, too, the processes are by which, even under favourable circumstances, this cicatrix tissue becomes assimilated to the structure of the organ in which it is developed, can hardly be better shown than by the following account by Dr Lange,[‡] of the examination of the uterus of a woman who died of some chest affection nearly two years after the performance of the Cæsarean section.

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[*] Whitehead, Medical Gazette, Sept. 1841; and Kranefuss in Rust's Magazin, &c., Bd. xlv, p. 257, as quoted by Kayser, p. 87.

[†] Many of these cases are detailed by Professor Michaelis of Kiel, in his valuable essay on the Cæsarean Section See his Abhandlungen aus dem Gebiete der Geburtshulfe; Kiel, 1833.

[‡] In a paper at p. 126, vol. iv, of the Vierteljahrschrift fur die gesammte Heilkunde, for 1846, which contains by far the best account of the structure of the cicatrix in these cases ever yet pnblished.

"Just over the symphysis pubis there was a deep depressed cicatrix several lines in depth, and of the size of a sixpence. From the inner surface of the abdominal walls there was seen continued from this cicatrix, a wedge-shaped prolongation, half an inch in length, as thick as the finger, of a round, but slightly flattened shape, extending to the anterior wall of the uterus, and being closely united to it. In the interior of this band there was a cavity which communicated with that of the uterus, by an opening two lines in diameter. The interior of this conical cavity was lined by a prolongation into it of the uterine mucous membrane, and there seemed, moreover, to be a process of reproduction of uterine tissue in course of progress from the basis towards the apex of the cavity. Several bands of a callous substance, resembling that just described, ran from the sides of the larger cicatrix to the anterior wall of the uterus."

Dr Lange remarks, concerning these appearances of which I have given an abridged description, that this sort of diverticulum proceeding from the uterus, indicates sufficiently clearly that the healing of the wound was not effected by union of its edges, but by the effusion of plastic lymph from the inflamed uterine peritoneum, and its cohesion with the inflamed abdominal peritoneum.

The surgeon who removes a limb in which gangrene has commenced, knows that, unless his incisions are made some distance above the decaying tissue, he can have no hope of the healing of the wound. The uterus, after delivery, is in a condition, not certainly of decay, but, at least, of disintegration. Instead of there being any natural tendency to an increased afflux of blood towards it, that first step towards the repair of injury, the very discharge of its contents cuts off half the supplies of blood previously directed to it. From the weight of xxiv. ounces, which the organ has been ascertained to have just after delivery, it is reduced, in the course of six weeks, to a weight not exceeding an ounce and a half. This change, too, is effected, not by simple absorption, but is attended also by a general degradation of its tissue, of which the abundant presence of fat globules in the lochial discharge, and in the *debris* which cover the interior of the organ, is ample evidence.* It is true, indeed, that, simultaneously with the removal of the old tissue, there goes on a formation of new; but of a tissue much more lowly organized, possessed of but small power to repair injury, and which commonly requires, for its higher development, the appropriate stimulus furnished to the uterus by pregnancy, and the sojourn of an ovum in its cavity.

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* See Virhow's account of the microscopic examination of the uterus of a woman who died fourteen days after delivery, in the Verhandlungen der Gesellschaft fur Geburtshulfe, in Berlin, vol. iii. p. xvii.; with which the brief account by Kolliker, in the Zeitschrift fur Wissenschaftliche Zoologie, vol. i., p. 73, perfectly harmonizes. The elaborate essay of Professor Franz Kilian, in Henle und Pfeuffer's Zeitschrift, vol. ix., p. 1, furnishes a more complete account than is elsewhere to be found, both of the development of the tissue of the uterus, and of the changes which take place in it after delivery; and demonstrates the fact of the removal of the old uterine tissue, and the production, in its place, of a new and more lowly organized tissue, to which reference is incidentally made by Mr Rainey in his paper in the Philosophical Transactions for 1850, part ii. pp. 519-20.

The condition of the uterus in the patient whose history I have related, gave an apt illustration of the correctness of these views. The organ was generally pale and bloodless, the edges of the incision through it were swollen, infiltrated, œdematous, and discoloured by altered blood, so as to look, at first, almost as if gangrenous; and I have little doubt but that, in many of the cases in which the organ was alleged to have been in a state of gangrene,* that condition was, as in the above instance, apparent rather than real.

If, then, such and so many dangers beset this operation, if the recoveries from it be so few, and the mortality so great, while the causes of that mortality are, for the most part, beyond the power either of surgical dexterity or medical skill to obviate, and some of them inseparable from those processes which needs must follow delivery, we may, I think, feel satisfied that the general rule in British Midwifery, which prohibits the performance of the Cæsarean Section, except where delivery would otherwise be altogether impossible, rests on a far sounder foundation than that of mere prejudice, or blind obedience to the dicta of men eminent in their profession.

At the same time, these facts can yield no apology for those who, in cases calling for this operation, allow, from any cause whatever, the right moment to pass by unused. They rather leave such conduct altogether without excuse, since, by it, dangers always great, are rendered insurmountable, and hopes, small at the best, are utterly destroyed.

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* Many cases of an analogous condition of the uterus, in women who have undergone the Cæsarean section, might be adduced. The following may suffice from the history of a woman who died thirty days after the performance of the Cæsarean section. "The wound of the uterus was slightly closed at its lower part; but at its upper end, where it had been carried quite up to the fundus uteri, it not merely gaped widely, but the absorption of its edges had gone on with such activity, that nearly the whole of the fundus uteri was consumed."—Busch, Geburtshulfliche Abhandlungen, 8vo, Marburg, 1826, p. 243. He attributes this condition of the uterus to the effects of the osteomalacia, which was still advancing at the time of the patient's death: but it may, I think, with more propriety, be referred to those causes mentioned in the text as acting on the uterus after delivery.

TABLE (referred to at p. 396.)

Showing the Cause of Death, and the Date of its occurrence, in 147 Fatal Cas's of the Cæsar·an Section.

Date of Death.	Hæmorrhage.		Shock to nervous system.		Inflammation.		Hæmorrhage and shock.		Hæmorrhage and inflammation.		Shock and inflammation.		Causes independent of operation.		Total.
	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	
Under 1 hour......	1	1	1	15*	...	...	...	...	...	...	...	...	...	...	[illegible]
Between 1 and 2...	...	..	...	..	...	...	...	...	...	...	...	...	...	...	[illegible]
„ 2 „ 6...	2	2–3	1	15	1	47	2	101–2	..	...	...	...	...	...	[illegible]
„ 6 „ 12...	1	4	...	...	1	48	...	...	...	...	...	...	...	...	[illegible]
„ 12 „ 24...	4	5–8	7	16–22	1	49	2	102–4	...	...	...	...	...	...	[illegible]
„ 24 „ 36...	2	9–10	6	23–28	11	50–60	...	...	2	110–1	1	128	1	139	23
„ 36 „ 48...	1	11	5	29–33	7	61–67	...	...	3	112–4	3	129–31	...	...	19
„ 48 „ 72...	2	12–13	7	34–40	7	68–74	2	105–6	4	115–8	2	132–3	..	...	24
„ 72 „ 96...	...	...	4	41–44	0	75–82 80*–81*	1	107	2	119–20	1	134	...	..	18
On 5th day	1	14	1	45	5	83–87	2	108–9	4	121–4	3	135–7	...	...	16
„ 6th „	...	...	1	46	2	88–89	...	...	1	125	1	138	...	[illegible]	[illegible]
„ 7th „	...	..	...	...	1	90	...	...	...	..	...	...	1	[illegible]	[illegible]
„ 8th „	...	..	...	...	4	91–94	...	...	2	126–7	...	...	1	[illegible]	[illegible]
„ 9th „	...	...	...	...	2	95–96	...	...	...	...	...	...	1	[illegible]	[illegible]
„ 10th „	...	...	...	...	...	...	...	...	...	...	...	...	...	[illegible]	[illegible]
Bet. 10th and 12th	...	...	...	..	...	...	...	...	...	...	...	...	...	[illegible]	[illegible]
„ 12th „ 15th	..	...	...	...	2	97–78	..	...	...	...	...	...	..	...	[illegible]
„ 15th „ 20th	...	...	...	...	1	99	...	...	...	...	...	...	...	...	[illegible]
Above 20..........	...	...	..	...	1	100	...	.	..	...	...	...	2	143–4	3
Total............	14		[illegible]		[illegible]		[illegible]		[illegible]		[illegible]		[illegible]		[illegible]

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ART. VIII.—*A Case of Cæsarean Section.* By Dr OLDHAM, Obstetric Physician and Lecturer on Midwifery, &c., at Guy's Hospital. (Received January 14th, Read February 11th 1851. Med.-Chir. Trans., Vol. xxxiv., p. 89.)

SARAH ———, aged 23, engaged at waistcoat making in Bethnal Green, was born a feeble child, and during childhood was weak and rickety. The mother states that she was seven years old before she could put her feet to the ground, and thirteen years of age before she could walk securely. At sixteen she first began to menstruate, and has continued to do so with tolerable regularity ever since, and her general health has been fairly good. She is now four feet eight inches in height, walks lame, and her legs are curved, so that she retains a deformed and dwarfish appearance. Her face and features are small, and her countenance is that of a girl of fourteen or fifteen years of age, and her circulation habitually rapid.

She applied to me for advice in the middle of September 1850, complaining of amenorrhœa of seven months' standing, the result, as she declared, of cold, for which she had ineffectually taken various medicines. At this time the abdomen was prominently large, and by auscultation the beats of the fœtal heart and uterine murmur were readily detected. She denied, at this time, the possibility of pregnancy, but subsequently she admitted that she had had sexual intercourse on two occasions, but she was in so much alarm at the time, that she thought it impossible that she could have become pregnant. The lower lumbar vertebræ and the sacrum were observed to be deeply curved, and the uterus was anteverted in a marked degree. On examination, the orifice of the vagina was found to retain much of the virgin contraction, from an imperfect laceration of the hymen; the canal itself was narrow and very tender, so that she shrunk from the necessary exploration. The projection of the promontory of the sacrum was so great as to shorten the conjugate diameter to about two inches; but the pelvic cavity and outlet were comparatively roomy. She was advised, without loss of time, to obtain a room near Guy's Hospital, and to be entered as a patient of the Lying-in Charity.

Sept. 23, 1850. This morning I determined to induce labour by puncturing the membranes, the bowels having previously been relieved by castor-oil. For this purpose a long slightly-curved canula, with a sharp-pointed stilette, which could be withdrawn at will from the canula, was employed. Some difficulty was experienced in reaching the *os uteri*, as the patient lay on her side, on account of the height at which it was placed, the extreme tenderness of her soft structures, and her inability to flex her thighs well, which was caused by the prominence of the anteverted uterus. She was therefore placed on her back, and the canula having entered the amnial sac, about eight ounces of clear *liquor amnii* were drawn off. At this time the fœtal head could be felt, distinctly marked out on the abdomen above the pubes.

24th. This morning the bowels had been again relieved by aperient medicine, and some uneasiness about the uterus had been felt. Mr Davies, who had charge of the case, examined and found the left arm in the vagina. On visiting her, I found that the child was still alive, and that touching the palm of the hand in the vagina, excited reflex muscular movement. The *os uteri* surrounded the arm, and the uterus was quiescent. The patient was ordered to keep quiet, and it was requested that no further vaginal examination should be made.

25th. She had not slept well on account of some irregular diffused uterine pain. The arm and *os uteri* were in the same condition. She had passed urine freely, and during the day was tolerably free from pain. Her pulse is rapid, 93 in the minute, which is natural to her; but there are no febrile symptoms, and she is in good spirits.

26th. Early this morning 6 drachms of castor-oil were given, which acted freely on the bowels in the course of the morning. At 9 a.m., some regular uterine pains commenced; and when I saw her the os uteri was found to be dilating well, and the vagina softer and less tender. The skin of the fœtal arm was peeling off.

Vaginal examination was forbidden.

At half-past 9 p.m., the os uteri was fully dilated, but the loose soft margin of it could still be felt, on account of the fœtal head being so held above the brim as not to have any bearing upon it. Some efforts at delivery were now made, the rectum and bladder being both empty.

A full exploration of the pelvic brim and the presenting part was made by the cautious introduction of the fingers of the left hand, with a view, if practicable, of running them beyond the brim, and catching and bringing down a foot; the right hand at the same time pressing the uterus from the abdomen, and doubling down towards the pelvic inlet the lower limbs of the fœtus, in a direction to meet the hand in the vagina. The patient was at this time on her back, but the pelvic brim was so contracted that it was soon obvious, that any hope of delivery in this way must be abandoned. Traction was then made on the protruding arm, which shortly gave way at the elbow-joint; but the efforts of traction were found to have some effect upon the head, n moving it from above the pubes to the brim, which was aided by external pressure, directing it from the one part to the other. After the arm had separated, the head was so far within reach that the perforator could be made to touch it, and it was opened without difficulty. After the brain had been emptied, and, under the influence of regular and powerful uterine action, the bones of the head had shrunk together, repeated efforts were made with the crotchet, applied within the skull and outside of it, to draw the head through the brim. In doing this the cranial bones and a part of the base of the skull were torn up, but without making any material way through the contracted brim. In the course of this operation, which was occasionally suspended for ten minutes to rest and refresh the patient, the right side of the pelvis was found to have squeezed into it the right hand and foot, both of which could be distinctly felt in this situation. A hope was now entertained that one or other of these limbs would double deeper into the pelvis, so as to be caught and drawn down, and attempts were made to accomplish this, but ineffectually. No hold could be got of either the foot or hand beyond the slippery tips of each, and labour-pains did not press them lower. A polypus canula and ligature were sent for, but the noose could not be passed over the ankle or wrist, and consequently there was no purchase for it. With the narrow brim of the pelvis thus packed by this complex presentation, the different parts of which mutually obstructed the process of each other's descent, and judging from the difficulties already experienced on attempting delivery—of the far greater ones which must be encountered in order to complete it—it became a question whether delivery should not be effected by the Cæsarean section. After a consideration of the case, I determined in favour of it; but before undertaking it, a consultation was held with my colleague, Dr Lever, who, having examined the patient, acquiesced in this decision, and without further delay preparations were made for performing it.

The room was heated by a good fire until the temperature was sensibly warm, some candles were obtained from the Hospital, and the Assistant-surgeon of the Hospital, Mr Poland, who operated, with the Resident Obstetric Clerks, soon arrived. The bladder was emptied by a catheter. The patient was placed completely under the influence of chloroform, and one gentleman had charge of the chloroform, and kept her during the operation under its power. The patient was drawn to the right side of the bed, the shoulders were slightly raised, and the best arrangements were made to let her maintain, without moving, the same position after the operation. One assistant was directed to close the abdomen as the uterus contracted on being emptied, and the act of delivery was undertaken by myself.

An incision beginning about two inches above the umbilicus, and carried down to five inches below it, so as nearly to reach the pubes, was made in the course of the *linea alba*, avoiding the umbilicus itself. No bleeding of consequence followed this incision, and the uterus was soon exposed. On opening the abdominal cavity, some three or four ounces of transparent straw-coloured fluid ran out, and were caught by a sponge. The body of the uterus was seen to contract in a perfectly even way on thus being laid bare, and to form a hardened compact body, its surface having its natural polish, and neither the large veins nor smaller capillaries being strikingly injected. The omentum glided easily between the *fundus uteri* and the abdominal wall, with a descent of about two inches. The uterine wall was then cut through, and the direction of the incision was slightly curved, the convexity looking to the right side. The length of the opening was about five inches, and the incision was made to reach the lowest part of the uterus near the cervix, so as to open the womb from below. But little blood was lost. I then passed my right hand into the cavity, and withdrew a full sized seven months' fœtus, by laying hold of its thigh. The uterus immediately contracted, and the intestines appeared at the upper part of the abdominal opening, which was promptly closed. The hand was again introduced between the membranes and the surface of the uterus, and with but little delay the placenta and membranes were removed, the former being attached to the upper part of the posterior wall. The uterus then contracted, and in doing so the convex border of the incised wall was seen to wrinkle, and not to adapt itself to the corresponding surface. Some venous bleeding took place, but to no great amount. A sponge wrung out of warm water was now gently drawn over that portion of the convex border of the intestines and omentum which had protruded and been smeared with blood, and the upper part of the external opening was at once closed by the application of sutures. Whilst this was being done by Mr Poland, I held a sponge over the edges of the uterine opening, gently pressing them together, and by the time that the wound was closed above, all bleeding had ceased, and the two surfaces were evenly adapted, and were seen to be quite free from any interposed structure. Other sutures, eleven in all, were then applied; some broad strips of plaster were drawn across the abdomen, and two cushions of lint were placed vertically on the sides of the central incision, and a binder cut out into tails was adjusted over the whole.

During the performance of the operation the patient was quiet, excepting when the uterus was at its height of contraction, when she wriggled about slightly, but hardly enough to incommode. Mr Poland. The pulse was of good strength, and she was left reclining on her back, and sleeping in a tranquil manner under the influence of chloroform.

At a quarter to four, A.M. three quarters of an hour after the operation, she opened her eyes as though waking from a natural sleep, and she then told me that she had known nothing of the operation, and felt quite comfortable. Just before this her pulse was 118 in the minute, and the respiration 32.

During the remainder of the night she took two drachms of laudanum; at first, one drachm was given her soon after she awoke, half an hour after which she began to shiver; the pulse became rapid and weak; the respiration quickened and catching; the extremities cold; and she complained urgently of an oppressive tightness over the upper part of the abdomen. A small quantity of hot brandy-and-water, and half a drachm of laudanum were given; and some warm clothes were placed around her. The bandage was loosened at its upper part, which gave her some relief. In an hour's time she was in a profuse sweat, and felt easy, but was indisposed to sleep. The remaining half drachm of laudanum was then given, and towards morning she had some sleep.

First day.—In the morning she expressed herself as feeling comfortable, and free from pain. She breathes easily, and the skin is freely perspiring. Pulse 130; tongue moist, slightly furred. The upper part of the abdomen, above the bandage, is distended with flatus, so as to bulge up above the edge of the bandage. Six ounces of dark coloured urine were drawn off by the catheter. The pupils are contracted, evidently from the effects of opium, and she is in a quiet drowsy state, readily, however, answering questions, and quite free from restlessness.

Great care was taken to keep her undisturbed, and she was ordered cold drinks of spring or toast-water, or tea. In the course of the day she was ordered—*Julepium Ammon. Acet. c. Mist. Camph. et. Tinct. Hyoscyam. quâque ʒss. 4tâ horâ.* But of this she took only two doses.

In the evening.—She had passed the day in a perfectly tranquil manner, sleeping a good deal. She had taken freely of the cold liquids from time to time, and there was but little alteration in her general state, which was as favourable as could be expected. Between six and seven ounces of clear urine were drawn off. She had sweated profusely at intervals during the day, requiring the clothes to be lightened, and now the skin is warm and moist. There is no abdominal tenderness.

The vulva was sponged with warm water; warm napkins were applied, and the sheets were shifted from under her with as little movement as possible. There was a moderate dark-coloured lochial discharge.—*Calomel. gr. j. Pulv. Opii, gr. ij. in formâ Pilul. Statim sumenda.*

Another pill was ordered to be given, at the discretion of the resident obstetric clerk who sat up with her, if the first should fail to procure rest.

Second day.—The second pill was given at 2 a.m., and she passed a tolerably quiet night. Towards morning she complained of a sharp shooting pain below the edge of the false ribs on the right side, with short, free intermissions. Pain also was felt in the uterine region, and the abdomen was more tympanitic. She had passed urine of her own accord during the night. Pulse 150, small; respiration more rapid; mind clear. The pain in the side increased towards noon, and the breathing was more embarrassed. Some cloths wrung out of hot water were applied to the side. Another dose of two grains of opium was given, and the vulva again sponged; she had partaken freely of barley- and toast-water, and tea; on one occasion she had vomited, but without effort. In the afternoon her breathing became catching, and more frequent; her pulse more rapid and feeble, and evidence of vital exhaustion was apparent. She died in the evening.

From the time of her delivery to her death, this patient was watched, without intermission, by Mr Davies and the resident obstetric clerks.

Post-mortem examination. Thirty-six hours after death an inspection of the abdomen was made by myself with Messrs Stocker and Morgan, the obstetric assistants. The upper region of the abdomen, above the bandages, was distended by flatus; but the middle and lower parts, which had been covered by plaster, were comparatively flat. On removing the bandage and plaster, the edges of the external wound were seen closely approximated; and on cutting through the sutures, the process of adhesion was found to have made some advance, so that it needed some little traction, beyond the weight of the neighbouring tissues, to reopen the incision. On exposing the abdominal cavity, a few coils of the small intestines, near the uterus, were seen not much injected, but with some films of recent lymph between them, and some serous fluid tinged with blood, and intermixed with flakes of lymph. But the degree of peritonitis was not great, and it did not extend to the upper part of the abdomen. The bowels were distended with gas, but their solid contents were trifling. The omentum was much thickened and indurated, and its blood-vessels were filled with blood. It sunk upon the *fundus uteri*, and there, being comparatively fixed, it formed a barrier, which prevented the blood from the open uterus running into the peritoneal cavity. The uterus occupied its normal position after delivery; rather inclined to the left, and well reduced in size. The length of the incision was reduced to three inches, and its curved outline more distinct than when made during the operation. The edges, for almost the entire length, were separated, leaving a gap of a half to three-fourths of an inch in width, when measured from the outer suface. The edges, towards the inner surface of the uterus, were nearer together, but still apart, and some lochial discharge ran out through the opening, which, during life, had apparently been retained within the limits of the uterus by the close fitting in of the abdominal walls with the pressure of lint and strapping, and the upper wall of indurated omentum. The length of the uterus was nine inches and a quarter, and if a line were drawn half an inch transversely above the centre, the incision would be contained in the lower part. The peritoneal surface of the uterus was more opaque than natural, with here and there some shreds of recent lymph; there was but little vascular injection. The muscular structure of the uterus, and the veins were not inflamed or thickened, excepting at the margin of the opening, where the tissue felt more dense and crisper than usual. The os uteri was uninjured. A corpus luteum was in the right ovary.

The pelvis, which I was fortunate enough to procure, is altogether *small*, all its diameters being below the standard measurement, excepting at the outlet, which is sufficiently spacious. The principal deformity is in the conjugate diameter of the brim, on account of the promontory of the sacrum jutting forward with an inclination to the *right* side. It measures two inches. From the promontory to the *right* pectineal eminence there is a space of two inches, but to the *left* pectineal eminence, there is two inches and an eighth. The transverse measurement is four inches and three-fourths, and the oblique four inches. The diameters of the cavity, although contracted, are not so reduced as to cause any serious obstacle, in a practical point of view, to the removal of a child.

Remarks.—Two practical questions were suggested by this case;—I. What was the best plan to attempt to follow out in the delivery at first? II. Were the complications such as, under the circumstances, to justify the Cæsarean section?

I. When first I saw this young woman, she was already seven months advanced in pregnancy; and my conviction, after a vaginal examination, was that there were, as nearly as possible, two inches in the conjugate diameter, which was *the* particular solid obstruction to be considered in the delivery.

The two methods of treatment which might be adopted, were —(1) to induce premature labour, and deliver by craniotomy, and (2) to allow her to go to term, and then perform the Cæsarean section.

(1.) By inducing premature labour, although at the age of of viability, there was no expectation of saving the child, but only that, by removing a body of relatively small dimensions, the operation by the crotchet would be facilitated. The important favourable circumstance in this mode of delivery was, that it afforded, in my judgment, a reasonable expectation of being completed through the birth-passages by an operation, from which the mother might escape with impunity, although at the sacrifice of the child.

In favour of the Cæsarian section, there was the probable preservation of the child, and the somewhat diminished risk from so formidable an operation, by a careful preparation of the patient for it,—undertaking it at a selected moment, and with such attention to points of practice during and after it, as have lately been found of service in abdominal surgery. This mode of delivery would, I believe, have found favour in the eyes of some obstetric practitioners, with whom the expectation of saving the child's life would have outweighed the degree of operative danger to which the mother would be exposed. My own judgment was clearly in favour of the practice which was adopted. Had the [illegible] been greater, and pregnancy, as in this case, been protracted to the seventh month, preventing, therefore, [illegible] there would have been no hesitation [illegible] the Cæsarean section. But with a [illegible] and a sufficiently spacious [illegible] there was [illegible] likelihood of delivering by the [illegible] in not undertaking [illegible] and [illegible] a serious surgical operation. I was [illegible] the maxim which [illegible] in England in favour of the [illegible] this case, when [illegible] and [illegible] the same practice. My [illegible] of the child, which was [illegible] so augmented the [illegible] my purpose.

[illegible] question, it will be noticed [illegible] the first stage of [illegible] the degree of [illegible] operation [illegible] by the same [illegible] successfully [illegible] some cases of unavoidable hæmorrhage, with a rigid undilated os uteri. Failing in this, the prolapsed arm was, with the aid of external pressure, made the means, by traction on it, of bringing the head over the brim, which allowed of its being opened, reduced, and drawn upon, until I hoped to get it through the brim. It was in a pause of this operation that, on sweeping the inlet of the pelvis with the finger, I found that a foot had descended by the side of the hand, and had been squeezed into the right side of the pelvis by the action of the uterus. On watching the effect of some labour-pains, I found that the force of the contractions was spent, not in pressing one part below the other, but in wedging them altogether in the narrow brim. It was obvious that, if either extremity could be dislodged from the brim and brought down, it might be made of great service in helping delivery; but all attempts to accomplish this manœuvre failed, as no sufficient hold could be got of them.

With this new source of difficulty, the question arose, what was best to be done? It appeared to me that one of two courses might be followed. The first was to persevere in removing the mutilated fœtus through the vagina; and the second, to deliver by the Cæsarean section. I felt, too, that it was imperative to decide between them *at once*, as the effect of every half-hour's delay, especially if employed in further efforts at extraction, would increase the danger of the Cæsarean section, and, indeed, if long persevered in, would compromise all chance of its success. The patient at this time manifested no signs of exhaustion, the pulse, though rapid, was of good volume; her spirits were fairly good; the abdomen was free from tenderness, and the sexual passages were not inflamed.

The principal reason which induced me to prefer the Cæsarean section was, that I feared the patient could not sustain such prolonged efforts at delivery, as would have been necessary, and that she might die exhausted before or soon after delivery. The girl had but a feeble constitution, although her health had not, of late years, been bad; and I had no confidence in its enduring against this hard trial of its powers. From the efforts I had already made, and from some experience in severe cases of craniotomy, I was quite sure that it would be a tedious and difficult task, although aided by a fast advancing putrefaction of the fœtus, to effect delivery; and that even the life of a strong woman would have been placed in peril by it.

I could not but call to mind a case which occurred in the Lying-in Charity of Guy's, in October 1835, at which I was present. The patient, aged 32, was in labour with her sixth child, and the right hand and arm descended at the commencement of labour. An effort was made to turn by the student in attendance, but ineffectually. She was bled to 16 ounces, and took 100 drops of laudanum, when another attempt at version was made by the assistant-accoucheur, which also failed. The pelvis was found to be contracted in all its diameters, and the conjugate diameter measured two inches ten lines. Eight hours from the first attempt at delivery, she was removed from her lodgings to a ward prepared for her in the hospital, and Dr Ashwell then took charge of the case. She had already had a rigor; the pulse was rapid, and the abdomen tender. Without trying again to turn, Dr Ashwell made attempts to remove the child by the cutting hook; but it could not be fixed upon the neck to sever the head from the trunk. The thorax was perforated; but after the most strenuous efforts, perseveringly followed out for many hours, it was obliged to be abandoned as hopeless, and the woman died undelivered. The pelvis is now upon the table. "If this case could again occur," says Dr Ashwell, (*vide* Guy's Hospital Reports, Vol. i,) "at the full period, and the knowledge now attained were possessed, there would, I think, be little hesitation as to the propriety of the Cæsarean section."

Another unfavourable circumstance in this case, in reference to persisting with the crotchet, was the state of the vagina. This canal has been noticed as retaining the structure of early age. It was closely ribbed throughout, very vascular, with a thin epithelial covering, and a feeling imparted to the finger of its being structurally weak and easily lacerable. This was remarked to those around me at the beginning, as a source of danger in a prolonged craniotomy case; and, excepting when the hand was introduced which lacerated the fourchette, this canal was very carefully guarded when the crotchet was being used. Should the patient survive the immediate dangers of delivery, I could not help feeling that there was a great risk of vaginal sloughing, with its revolting consequences.

II. On the other hand, the Cæsarean section offered a speedy and sure, instead of a prolonged and doubtful delivery. It was not yet forbidden from exhaustion or any signs of inflammation; on the score of suffering, it contrasted most favourably with the other practice, and I thought, upon the whole, that the chance of ultimate success from it was greater.

The operation was performed much in the usual way without difficulty, and with nothing untoward. The extent of the reparation of the external wound, and the admirable manner in which the structures were laid out, to limit the mischief from the open wound in the uterus, and prevent its extravasated contents from being diffused in the abdominal cavity, are worthy of attention, and must have been greatly favoured by the perfect tranquillity of the patient under the influence of opium.

PART II.

CRITICAL ANALYSIS.

Art. I.—*A Practical Treatise on the Diseases Peculiar to Women. Illustrated by Cases derived from Hospital and Private Practice.* By Samuel Ashwell, M.D., Member of the Royal College of Physicians, London; and late Obstetric Physician and Lecturer to Guy's Hospital. Third Edition. London, 1848. 8vo, pp. 772.

This work appeared first in the year 1843 in parts, and in its complete form in November 1844; and in no long time it was honoured with the approbation of competent judges, both in this country and in the United States, for the large amount of practical information which it presented.

A second edition appeared in November 1846; and the present edition, which is the third, has been before the profession since June 1848. It was our intention before this time to have directed the attention of our readers to a work possessing not a few qualities of great value and undoubted utility. But various circumstances have hitherto concurred to prevent us from undertaking a duty, which ought to be performed in a manner becoming the nature of the Treatise.

We propose at present to advert to the characteristic properties of the Treatise of Dr Ashwell, as a guide to the practical physician, in an important class of diseases, upon the pathological nature of which different opinions are entertained, and which it is in some instances not easy to treat with certainty and efficiency.

Dr Ashwell distinguishes all the diseases peculiar to females, into two general divisions; the first the Functional, and the second the Organic Sexual diseases.

He allows the great difficulty of forming an accurate diagnosis in the former order of affections. In truth, the difficulty consists in determining what is a functional disease, and in many instances in saying whether any given set of symptoms is dependent upon what is called mere dynamic disorder, or may be connected with changes in the structure of the part. In order to clear away this difficulty, at the outset, Dr Ashwell defines a functional disease to be one *which is dependent on deviation from the natural or healthy action of any part of the organization, indicated by symptoms during life, which, on examination after death, are found to be unconnected with any discernible change in structure.*

Functional or Dynamic Uterine affections, Dr Ashwell observes, are mainly dependent on the derangement of menstruation; such, for instance are Chlorosis, Amenorrhoea, and Dysmenorrhoea, profuse Menstruation or Menorrhagia, and in some instances Leucorrhoea. In all these disorders a deviation from the standard of uterine health is observed to take place. These deviations are evinced in the absence, the scantiness, the excess of the uterine secretion, or in the substitution of a mucous, a muco-gelatinous, or a muco-purulent secretion instead of the normal one.

Dr Ashwell states it as the result of his observation, that, in judging of these deviations, it is less important to attend either to the amount or the quality of the uterine secretion, than to the regularity in the periods at which it takes place.

In estimating the influence of the state of this secretion upon the health of the individual, he adheres to the opinion that it is peculiar to the human race, and denies entirely the truth off the statement maintained by some, that a similar secretion takes place in the females of certain of the ape tribe.

With the affections now mentioned structural changes are not necessarily connected. Excepting the undeveloped condition of uterus and ovaries in chlorosis, anatomical inspection after death does not present any change, sufficient to account for the derangements often considerable in the health of females suffering under these disorders.

Even in the case of protracted and incurable Menorrhagia, Dr Ashwell observes, which occasionally terminates life by giving rise to dropsy and similar secondary diseases, if there have been no morbid growths co-existing, the uterus yields not any evidence of much structural disease. Increased softness and paleness of the uterine parenchyma and lining membrane will generally comprise the whole of the visible organic change. The same may be said of Amenorrhoea and Dysmenorrhoea. Beyond Hypertrophy and congestion in the latter affection, the organization generally continues unaltered.

It seems scarcely reasonable to admit, that the presence of the conditions last mentioned are competent to prove entirely the position of the author, that there is no organic change. Anatomists and pathologists are not in all instances well agreed as to what constitutes an organic change. But we suppose that not a few would say that hypertrophy is a certain degree of change in structure; and even some might be found who would allow that congestion is indicative of a slight change in organization. We here would make a distinction between organization, properly so called, and structure, with the view of marking more correctly than has hitherto been done, the transitions from changes in function to absolute changes in structure. The organization of a part consists in its bood-vessels, lymphatics, nerves as well as its proper tissue. Any changes in the blood-vessels, in the kind and amount of blood which they receive and convey, in the size and number of the vessels, should be referred to the head of changes in organization. Under this head, accordingly, congestion might be arranged. But changes in the blood-vessels, it is well known, lead, if sufficiently intense and long continued, often to changes in the structure of the part; and these might be distinguished altogether from the mere changes in organization. Hypertrophy, it appears to us, must in most instances be regarded as a change in structure. The part, for instance a muscle as the heart, a gland as the prostate gland, or the brain, becomes not only enlarged by the interstitial addition of matter, but it is denser and firmer and harder than in the healthy state.

We are obliged, therefore, to differ from the author on this subject, and to say that, if in Dysmenorrhœa, the uterus is in a state of hypertrophy and congestion, that disorder can no longer be reckoned as one merely functional.

Personal observation and experience even compel us to advance a little farther, and to say, that, in a certain class of cases of Dysmenorrhoea, some of the tissues of the womb are in a state of chronic congestion, which may proceed to chronic inflammation and hypertrophy of the organ; and in this class of cases we have always thought that the dysmenorrhoea is a mere symptom and effect of the morbid state of the womb. In such circumstances, the uterus is found upon examination to be heavy, painful, hot, and swelled, especially at the orifice. There is in truth from some cause or causes, varying probably in different cases, a larger than normal and necessary quantity of blood flowing into the vessels of the womb, and remaining in these vessels, so as to give rise to the vascular congestion, and the consequent hypertrophy. The objection now stated, Dr Ashwell may, no doubt, obviate by saying, that Dysmenorrhoea in such circumstances, becomes symptomatic, whereas he is treating of it at present as an idiopathic disorder. This reply is certainly admissible. But the very circumstances show that the plan of regarding Dysmenorrhoea as a disorder solely dynamical is not free from objection. It is also no slight confirmation of the validity of the objection, that in the subsequent page, Dr Ashwell states, that "Dysmenorrhœa appears to be most frequently induced by irritation more or less acute, by a low inflammatory state of the uterine mucous membrane, or by an active or even slow congestion of the uterus."—(P. 4.)

The proximate cause of these so-named functional disorders has never been well understood, and has consequently given rise to controversy and difference of opinion. Dr James Hamilton, Senior, it is well known, without being much of a theorist, was rather disposed to deny the influence of the state of the ovaries and uterus in the production of chlorosis; and his experience led him to ascribe the disorder chiefly to constipation. This inference, Dr Ashwell thinks not well founded; and he thinks it is more consonant to observation, to ascribe the phenomena of chlorosis to lack of energy in the ovaries, and to regard the constipated state of the bowels, as well as the chlorotic symptoms, as the joint result of this defective energy.

To the hypothesis that menstruation depend upon the orgasm of the ovaries exciting the circulation and action of the uterine vessels, Dr Ashwell gives merely a passing attention, to say, that a larger amount of evidence is required before assent can be yielded to this doctrine. The author mentions in a note the circumstances of three instances, in which he had examined the bodies of females who had died, while the process of menstruation was present, and in which, though the ovaries were carefully examined, and were found vascular, yet no cicatrix or appearance was found indicating that any Graafian vesicle had recently been discharged.

In conformity with the division of Uterine Diseases, established by the author, the work is divided into two parts. The first devoted to the Functional Diseases of the Uterine System; and the second to the Organic Diseases of the Internal and External Female Organs of Reproduction.

The First Part is subdivided into eleven chapters, in each of which Dr Ashwell treats of the following subjects:—

I. Chlorosis; II. Amenorrhœa; III. Vicarious Menstruation; IV. Dysmenorrhœa; V. Formulæ for Remedies; VI. Menorrhagia; VII. Leucorrohœa; VIII. The Disorders attendant on the Diseases of Menstruation; IX. Formulæ for Remedies; X. Hysteria; and, XI. Irritable Uterus.

The Second Part is in like manner subdivided into eight chapters, in which the following subjects are successively treated.

I. General Remarks on the History and Symptoms, Diagnosis, Pathology, and Prognosis in Organic Diseaseas of the Uterine System; II. On the Tumours of the Walls of the Uterus, characterised by Induration; III. On Premature Labour in Pregnancy, complicated with organic Diseases; IV. Organic Diseases of the *Cervix* and *Os Uteri;* V. Organic Diseases of the

Mucous Membrane of the Cavity of the Uterus; VI. Displacements of the Uterus; VII. Diseases of the Ovaries; and, VIII. Diseases of the External Organs of Generation in the Female; followed by an Appendix on the Morbid consequences of undue Lactation.

I. Of *Chlorosis* the author gives a good account, both in its simple and its complicated form.

The pathological cause of this state of the system, he conceives, to consist in a morbid condition of the blood, which secondarily affects the ovaries and uterus by retarding their growth. At the same time he does not think this state the same as *Anaemia*, or the defibrinous state of the circulating fluid, but he allows that to this state it is closely allied.

His ideas may be most easily understood from the following passage on the diagnosis of the disorder.

" *Diagnosis.*—It will be allowed, after what has been advanced, that chlorosis is not an inflammatory disease. It is indeed most intimately connected with anæmia,[*] and closely resembles (except in the pale greenness or dinginess of the skin,) that state of system which is the result of hemorrhage or any undue secretion. Those symptoms, therefore, such as severe headache, pain in the side affecting the breathing, distressing action of the heart, and several others, must not and cannot, where proper caution is observed, be mistaken for and treated as acute inflammatory affections. From the want of this caution, I have witnessed the very injurious consequences of such mistakes, the practitioner having forgotten, what in female disease it is peculiarly important to remember, that the severity of the pain, and the rapidity of the pulse, are generally indications of irritability and excitement, not of inflammation; demanding narcotics, carminatives, and at the most, counter-irritation, not bleeding, active purgatives, or spare diet. If my definition be correct, amenorrhœa, which may and often does exist, quite independently of chlorosis, cannot be regarded as a synonymous disease. It is impossible, that there should be chlorosis without more or less of amenorrhœa, yet it is often seen, that there is amenorrhœa without any degree of chlorosis. It is quite true, that when amenorrhœa has not been cured or relieved—when it has produced, by its long continuance, extensive constitutional derangement—that then chlorotic symptoms will appear, and that, ultimately, the series of morbid phenomena constituting chlorosis will be clearly seen. Nor must it be forgotten, that chlorosis is itself a frequent cause of amenorrhœa, by its prevention or protraction of puberty, on which menstruation depends; and subsequently, even when the catamenial function has been established, leading to its partial suspension, and occasionally to its entire suppression. There will be no difficulty in distinguishing chlorosis from chronic affections of the abdominal viscera, especially from hepatic, splenic, and renal disease, or indeed from any morbid state, of which anæmia and pallor are prominent symptoms.

The period of life, the peculiar aspect of a chlorotic patient, and the derangement of menstruation, will ensure a correct diagnosis."—Pp. 10, 11,

Of Complications, six are mentioned. First comes that of *Chlorosis* with *Amenorrhœa*; secondly, *Chlorosis* with *Hæmatemesis*, or discharge of blood from the stomach; the third, *Chlorosis*, with chronic derangement of digestion and nutrition; the fourth, the complication of chlorosis with dynamic affection of the brain and spinal chord, embracing *cephalalgia*, mental alienation, hysteria, epilepsy, chorea, and certain forms of paralysis; the fifth, affections of the vascular system and secreting organs, including *askites;* and, sixth, the complication of chlorosis with structural change in the lungs, sometimes proceeding to phthisis. The notices on these different complications, though not full, are sufficient to guide the practitioner, and to show the leading grounds upon which treatment should be conducted.

In his therapeutic and prophylactic directions, Dr Ashwell has been guided chiefly by the comparative frequency and importance of the complicated forms of the disease. On the subject of the last form of complication, the following remarks will show what is the degree of importance which he attaches to it, and the mode in which it should be treated, is either by prophylaxis or cure.

"Many such cases are occurring; and I wish the attention of practitioners to be particularly directed to this complication. The vicissitudes of an English climate predispose to phthisical disease; and with the physiological circumstances peculiar to the sex, explain how it is, that girls so frequently die, at this epoch, of phthisis—in connection with chlorosis and amenorrhœa. If asked, what such a series of symptoms as I have just enumerated indicate, the reply must be, chlorosis complicated with a tendency to phthisis. And if it be inquired, what is the chance of entire recovery, a very guarded answer must be given.

"To say that patients advanced thus far never recover would be untrue; although it is perfectly right, to give a doubtful opinion. If the constitutional power can be augmented, if the blood can be improved in quality, and increased in quantity, then the symptoms may be arrested, and renovation of health may be slowly effected. A symptom of improvement, of great value is, diminution in the rapidity of the pulse; for so long as the pulse beats 130, 120, or even 110 in the minute, it must not be supposed that any real amelioration has taken place. It will be necessary, also, to be guarded in the opinion we ourselves form; the same self-delusion exists here, as in phthisis at other periods. The patient is convinced, there is no occasion for alarm; and the disease often creeps on so insidiously, as to lead the family to believe, that there is no immediate danger. Let it, however, be remembered, that so long as there is a rapid pulse, short hacking cough, and a want of nutrition, there is real hazard. If, on the contrary, the pulse becomes slower, fuller, and softer—if the cough be less frequent—if the pyrexia disappear —and especially, if the patient gather flesh, in ever so trivial a degree—hope may be entertained.

"Carefully treat such an individual; avoid mercury, drastic purgatives, and emmenagogues. Place her in the country, where she shall breathe pure air: let her diet be simple and nutritious (milk and animal food), and her medicine some of the various tonics; and the expectation may be cherished, that the time is not far distant, when the sexual character will be fully developed, and the danger safely passed. From what has been stated, it must not be inferred, this is the only fatal complication of chlorosis; but, comparatively, that it is rare for the others to terminate unfavourably. Still, after continued derangement of the viscera of nutrition and digestion, the debility, pyrexia, and emaciation may become intimately blended with alteration of the pulmonary structure; and the cough, expectoration, and morning perspirations, may become prominently influential in bringing about final sinking.

"I have thus attempted to distinguish the morbid circumstances appertaining to the different forms of this prevalent malady. And although the leading and distinctive features will generally enable us to determine the complication; still, when any form of the disease has become aggravated, severe and of long duration, the blending of symptoms may perplex the diagnosis. It ought to be remarked, that leucorrhœa, in various degrees, is an almost constant attendant on chlorosis and amenorrhœa; and, when excessive, so seriously impairs the restorative powers, as to render the cure long and difficult.

"I wish to particularise, as correctly as I can, the ages, at which these various complications most frequently occur. Chlorosis alone, independently of amenorrhœa, is a disease of early life. In conjunction with menstrual suspension, it may be met with at any period, between the ages of puberty and the final cessation of the catamenia. Chlorosis conjoined with phthisis, may be seen between puberty and thirty years of age; sometimes later; but such instances are very rare, in comparison with the numerous complications of this kind, before the attainment of the twentieth year. Again, chlorosis with amenorrhœa or phthisis, at an early age, are forms of the malady, generally associated with debility and delicacy of system; while the other complications may exist at any period, and are not unfrequently combined with plethora or congestion."—Pp. 22, 23.

It is impossible to doubt the frequency with which this complication takes place, and the difficulty that in certain cases arises of distinguishing it aright and applying the proper treatment. This difficulty is considerably increased in consequence of the fact, that phthisis itself, not only in its approach, assumes something of the appearance of chlorosis, but is often preceded by amenorrhœa, or gives rise to that condition. Conversely, in all chlorotic cases, there is more or less dyspnoea very similar to that of phthisis, and not unusually cough. Indeed, the two affections so closely resemble each other in certain females, especially about the time of puberty, or shortly after that period, that it is often difficult to say, whether any given case is one of pure chlorosis or pure phthisis primarily, or of chlorosis threatening to proceed to phthisis, or of phthisis giving rise to chlorotic symptoms. Physicians of considerable skill and experience we have seen prescribe the use of iron in phthisis, upon the idea that the case was a chlorotic one or a chlorotic complication; whereas, had they been quite certain it was not, they would probably have withheld the mineral, and trusted to remedies less calculated to induce vascular irritation and excitement.

While the importance now stated both as to frequency of occurrence and the danger of the complication must unquestionably be conceded to this pulmonary form of the disease, it must not be kept out of view, that the fourth complication, namely, that in which the nervous system is more or less affected, is also one of no uncommon occurrence in certain classes, and one of very great importance as affects the subsequent health of the individual. It is in different classes of society produced in different modes.

Among the more easy classes it is probably induced by neglect of proper education, both physical and moral, by habits of corporeal inactivity, late hours, and many of the customs and rules observed in young ladies' boarding-schools and similar seminaries. The great desire at present, and for many years past, to cultivate the mind, and acquire accomplishments by incessant labour at music, drawing, and similar pursuits, carried on within apartments not always very well ventilated, at the expense and to the detriment of the corporeal health, manifestly contributes in a very decided manner to induce one or other of the forms of this complication. The mind and fingers are over-worked; and the body is inadequately exercised.

Among the working and inferior classes, again, it is incessant hard labour, scanty and innutritious food, with late hours, and want of sleep, or at least too short a period allowed for repairing the exhaustion consequent on the toil of the day, that not unfrequently are the antecedents of one or other form of this complication. In large, and populous, and middle-sized towns, there is a class of domestic female servants, who are not unusually victims of this complication, or some similar disease, produced in the same manner. The single servant of all-work, in a large or small family, with not very large means, has indeed a hard and miserable form of existence. She is expected to work the whole day, in various duties, continuous and laborious, and to extend her labours far into the night or early morning, in washing, or some other occupation, often on very scanty and inadequate fare; and then retiring to rest, she is understood and compelled to rise after a few hours sleep; and day after day, for weeks and months, to pursue the same laborious and uninterrupted routine.

In such a course the individual, though she had sufficient food, has not time to digest it. She has not even leisure to attend to personal cleanliness; and too often, if exposed to cold or moisture, she contracts some form of chronic disease, which, in an exhausted and worn out frame, lays the foundation of permanent bad health. It is not wonderful that these young women suffer, some from amenorrhœa, some from chlorosis and amenorrhœa, many from downright *anaemia* and debility, while several become attacked with epilepsy, palsy, and anomalous affections of the nervous system. After the animal frame has been long taxed with this excessive labour, with inadequate food and rest, it requires but a very slight disturbing cause, to induce in the nervous system such changes as to subvert altogether its power over the muscular system. That this picture is not overcharged the medical officers of the hospitals and dispensaries of large towns can testify in the amplest manner. These single servants of all-work are in truth in a worse condition than the hardest worked factory girls; and, occasionally, the condition of the servant of all-work is that of the domestic servant in situations, which externally at least present a more favourable appearance.

In looking to the treatment of chlorosis and its complications, Dr Ashwell studies to accommodate the treatment to each sort. Thus, in mild simple chlorosis, which, however, is not a common circumstance, he recommends that the remedies be principally of a constitutional kind, directed to the improvement of the general health, and the establishment of puberty. In the complications the treatment must be modified. It should be not only directed to the general or constitutional powers, but to the amendment of the local disorder which accompanies the chlorotic state. In instances of suspected or proved structural disorder again, a method still more cautious and more modified becomes necessary.

In the following passages Dr Ashwell states the general principles on which treatment ought to be conducted, and makes some useful observations on the methods, both prophylactic and therapeutic, which ought to be pursued with the twofold object of preventing the approach of the disease, and of curing it when it has taken place.

"The treatment of the most common form of chlorosis, namely, that accompanying puberty, may be regarded as the type of the treatment of all the others; embodying the principles, which, with greater or less modification, are universally applicable. It is here, at the very threshold of the disease, when its character is not understood, or when it is treated empirically, that the greatest error is committed. It is viewed as a local, not as a constitutional affection; and many are the individuals who have been sacrificed to the vain and ignorant attempt of prematurely establishing menstruation; mercury, drastic purgatives and emmenagogues, having irretrievably destroyed the constitutional power and paved the way for phthisical disease.

"It is not my intention elaborately to comment upon certain great mistakes in the *physical education* of female youth, And yet, I must be excused, if I direct attention to the diet, air, exercise, and clothing of the sex. It will readily be granted, that if, in these particulars, there is extensive deviation from the dictates of nature and common sense, there must be a proportionate risk of debility and disease. In our own changeable climate, it behoves the guardian of female youth to be especially prudent; and I am one of those who think, that it is scarcely possible to study these matters too closely. If the national practices in these particulars could be changed—and the remark applies with great force to the middle and higher classes of society living in cities and towns—chlorosis, imperfect puberty and amenorrhœa, would be uncommon, instead of being, as they are now, extremely prevalent diseases.

"Chlorosis is a rare affection in rural districts; where female youth are much in the open air, where it is not unfashionable to walk and run, and where it is not considered a gross violation of good breeding to sport and play with activity and vigour. Such girls acquire energy of system, each organ is developed, the blood is abundant and of excellent quality; nutrition is healthy, and puberty is attained without difficulty.

"These remarks may serve as an illustration of the principles on which the treatment of simple and amenorrhœal chlorosis must be conducted: and while it is scarcely possible to present a succinct and specific history of the pathology of this and the other complications, it is not difficult to describe, with simplicity and tolerable accuracy, the order of morbid events and the medicinal means, by which they are to be relieved if not cured.

"I have already observed, that a morbid state of the blood, of which anæmia is the prominent feature, lies at the basis of the disease. This may be viewed as the clue, by which the intricacy of the symptoms may be unravelled; and it will equally explain the nature of the malady, whether the specific morbid impression be in the system generally, in an isolated organ, a particular texture, or in any of the fluids of the animal economy.

"But, to be more precise. I would commence the treatment, by special attention to the digestive organs and alimentary canal; for I regard the disorder of these, as second only in pernicious effect, to the peculiarity of constitution, already mentioned. Nor will the advantage of their improved condition be limited to themselves; the deteriorated quality of the blood and its defective quantity, may both owe their origin to impaired digestion and nutrition. I have already alluded to the jaundiced hue of the complexion and of the surface generally, as leading to the suspicion of hepatic disease. The diagnosis will be made, by a careful examination of the region of the liver itself, of the urine and the fæces; which will prevent the possibility of being misled by the colour of the lips and conjunctivæ.

"At first, then, a due evacuation of the bowels must be daily secured; and much will depend on the kind of medicine by which this is effected. If mercury and drastic purgatives be frequently and largely employed, intestinal irritation will ensue, evidenced by unhealthy and undigested motions, mixed with mucus, and occasionally with blood. If the purging be excessive—if it be exclusively relied on for the cure—debility and exhaustion will be the result, and, in place of amelioration, the whole of the symptoms will be aggravated and severe. The best aperients are aloes, rhubarb, the sulphate of soda and manna, and if an alternative be necessary, the hydrargyrum c̄ cretâ. Nor must we forget that injection of a pint of warm water two or three times a-week, into the rectum, is, of all measures, the most efficacious in aiding peristaltic action, and in removing the load of the large intestines. The compound decoction of aloes with the compound tincture of cardamoms; the compound aloëtic pill with the oil of cassia and hyoscyamus, and the vinum aloes with the compound tincture of rhubarb, are the forms of these medicines I prescribe. The combination, with any purgatives or aperient remedies, of mild cordials, is exceedingly important."—Pp. 25-27.

Other particular remedies are chalybeate waters, which the author allows to be efficacious, and the use of iron which he thinks alone to be an efficient emmenagogue. The use every night of the hip mustard bath and the local salt shower bath across the loins, topically affects the uterus and induces the catamenial secretion. An injection consisting of one drachm of the pure liquor ammoniae to one pint of milk daily injected into the vagina, he mentions as a serviceable remedy. Iodide of iron he conjoins with tincture of Calumba or Gentian. Of the application of leeches to the mammae, which was a favourite method some years ago, and sinapisms to the same glands, he speaks doubtfully. But electricity, as a local uterine stimulant, he allows to be, in many instances, decidedly beneficial.

II. AMENORRHŒA.—The absence of menstruation is distinguished in the usual manner into the *Amenorrhœa* of retention and the *Amenorrhœa* of Suppression.

Amenorrhœa by retention is subdivided into three forms; 1. Amenorrhœa, dependent on congenital deficiency, malformation, or structural disease of the generative organs; 2. A, where, independently of deficiency or malformation, puberty is either slowly and partially developed, or is not developed at all; and 3. A, taking place after puberty has been fully established.

The Amenorrhœa of Suppression is distinguished into two varieties; 1. Recent and acute suppression and of chronic suppression.

On the first form and variety, or that dependent on congenital deficiency, malformation or structural disease of the organs of generation, Dr Ashwell remarks that it is now almost universally acknowledged that menstruation, as well as conception, depends on the existence and influence of the ovaries. In instances, therefore, in which these organs are either wanting, or considerably diseased, the case is irremediable.

> "The history of these cases is not encouraging: the health often suffers, and there is a proneness, either to irritability and excitement, or torpor and depression. I have now under my occasional care, a lady of thirty-two years of age, who has never menstruated, I believe, from congenital deficiency of the ovaries, and she is never quite well. Of late, her health has been more seriously deranged; she loses flesh, has frequent febrile attacks, a troublesome cough, pain in the side, and embarrassed respiration. The probable termination of this unhappy condition is phthisis. In this instance, sexual development and feeling are entirely absent; nor has there ever been leucorrhœal discharge."—P. 57.

It is well known that in cases of this kind the health is much affected. The sexual peculiarities of the patient often disappear or are changed; the voice is rough and hoarse; and more or less of a beard appears on the upper lip and occasionally on the chin.

Absence of the uterus is another organic cause of Amenorrhœa, which is equally incurable. Or if the entire organ be not wanting, the cervix may be deficient, the orifice impervious, or the vagina may be absent, imperfectly formed, unconnected with the uterus, or its sides may be adherent.

Dr Ashwell properly distinguishes between those instances of amenorrhœa in which the uterus and ovaries are absent, malformed, or diseased, and those in which these organs are present, but there is some malformation or obstruction in the vagina. In the latter class of cases the secretion takes place, but is not expelled, in consequence of the vagina being either closed by adhesion of its sides or occluded by an adventitious false membrane.

He allows that, for the treatment of cases of this order, it is impossible to lay down precise rules, and not free from peril to practise according to such rules.

> "Each case must be considered alone; its peculiarities must be coolly reflected on; and, while temerity is to be condemned, enterprise, short of recklessness, where the danger of non-interference is so great, is deserving of praise.
>
> In the simpler obstructions, the operations, either by the trocar, knife, or bougie, are not difficult; but it must not be forgotten, that such patients, with whatever facility the impediment may have been removed, and a mere incision is often sufficient, are really exposed

to the danger of peritoneal inflammation. When the operation has been successful, which it almost invariably is, in the simpler and more frequent cases, a quantity of dark, uncoagulated secretion escapes, and continues to drain away for several days. At length the uterus is emptied, and under favourable circumstances menstruation will occur naturally at the next, or at a more deferred period.

"Peritoneal or local inflammation, especially the former, must be carefully guarded against; and where it is necessary to keep the canal open, by bougies, sponge tent or dossils of lint, the earliest approach of abdominal or local tenderness must be promptly treated. After such a warning, every expedient for preserving the aperture must be discontinued. If the inflammatory symptoms are slight, local depletion, by cupping on the loins or hypogastric region, leeches, numerously applied, purgatives and narcotic fomentation or poultices may suffice; but if the pulse be full and hard, the skin hot, and the abdomen really tender, in a word, if there be peritoneal inflammation, nothing short of large and repeated general bleedings will avail as preliminary to these milder measures."—P. 60.

Amenorrhœa taking place after puberty has been established is probably the most usual and frequent form in which the disease is seen. This form, the author distinguishes as it takes place (*a*) in the plethoric, and (*b*) in the delicate, irritable, and hysterical.

In both these forms all the essential organs are present; but, from some peculiar causes, they either are unable to perform their functions, or they do not perform them. In the first class, there is little doubt that the main cause of the suppression is a congested state of all the organs, but especially of the uterus; and the author accordingly recommends in this form the employment of local depletion and the use of laxative and purgative medicines. Blood drawn by cupping from the loins or the sacrum, leeches applied to the labia, the inner and upper surface of the thigh, the groins, and the *os uteri*, are generally adequate, and give decided relief both to the pain in the head, back, and loins, and also to the local congestion. With regular but gentle evacuation of the intestinal tube it is of great moment to conjoin such measures as the sinapised hip-bath at 96° or 98° every second night, or even every evening. In this bath the patient should be kept half an hour, one hour, or even two hours, taking care to keep up the temperature. He also very properly insists upon combining the use of active exercise, by walking or similar methods. Horse exercise we have seen of much benefit in this form of the complaint. Dr Ashwell very properly prohibits the free use of animal food, malt liquor, and wine, in this variety. Indeed, in most instances, the remote causes of the disorder consist in the patient eating and drinking too much, taking too little exercise and either remaining within doors too much, or going out only in a carriage.

The following observations deserve the attention of the practitioner.

"The condition of the system may have been altered and improved, the treatment may have removed the plethora, but there is yet no menstruation. It may be asked, whether it be necessary in such circumstances at once to employ emmenagogues? I think not. Some months may elapse before the uterus shall perform its proper function, but eventually, menstruation will most probably occur. If, however, the health fails, and instead of a ruddy and robust, there is a pale and wan countenance, and a gradually pervading debility, the amenorrhœa will merge into chlorosis. To prevent such a termination emmenagogues may be used. But I must also observe, that if, when the plethora is removed, menstruation does not quickly occur, the continuance of the malady must not always be attributed to debility. Such an opinion leads to the premature and injurious exhibition of tonics and stimulants; for although weakness is a cause of amenorrhœa it is by no means its only condition, since often, where debility has been entirely removed, menstruation has failed to be established."—P. 65.

It appears, in short, that there is in this class of cases what Dr Parry and other physicians call Irregular Determination of Blood. It is probably neither the quantity nor the quality of blood that is in fault. But the circulating fluid is not carried in the proper channels. Instead of being distributed in proper proportions to the skin and extremities; the brain, the lungs, the liver, the kidneys, the ovaries and uterus; it is sent in too great quantity to one or two organs, and in too small proportion to another. In some it appears to cause violent headach and hysteria by its affection of the cerebro-spinal system. In others it causes cough and hæmoptysis by its determination to the lungs; in some it causes hæmatemesis; and in almost all, the blood which ought at each monthly period to be employed in filling the uterine vessels, and forming the catamenial secretion, is diverted and misapplied, in maintaining a constant catarrhal discharge from the vagina, or the utero-vaginal mucous membrane. The great object, therefore, in treatment ought to be, to re-establish the normal distribution of the blood, to restore that balance between the circulation in different organs which has been for the time subverted.

These views are true and applicable even to the second variety of this form of amenorrhœa, that, namely, which takes place in delicate, irritable, and hysterical females. For in these, though the state of the system be at first different, and the remote causes may also differ, yet both tend to induce, by different modes, a similar condition of the circulating system; and marks of irregular determination are not less manifest in the latter than in the former class of females.

Next comes the *amenorrhœa* of acute suppression, where menstruation, having been established, and continued for some time, has, independently of pregnancy or lactation, become suppressed. This may be either recent and acute, or chronic.

" The *two* great causes of acute suppression are mental emotion (especially of a depressing and anxious character), and the application of cold. Sexual intercourse during menstruation, fever, either idiopathic or secondary, hemorrhage or venesection, severe trying or drastic and emetic medicines, iced water and confectioners' ices are auxiliary and less frequent causes. The effect of cold in suddenly arresting menstruation is well known; whether it be applied by a stream of cold and damp air, by wet feet, by drinking cold water, when hot, or by undried linen. Nor are we less familiar with the injurious effects on the sexual functions of joyous and painful emotions. Not only is the secretion of the catamenia prevented, when about to occur, but when menstruation is present it is often immediately checked by sudden terror. The same observation is true also of the secretion of the milk. Happily the effect of several of these causes is diminished by the frequency of their occurrence. The bathing women go into the sea, during menstruation, with perfect impunity; and the habitual exposure to the casualties of life necessarily diminishes their injurious impression. Dr Gooch relates that a patient of his consulted him, long after the entry of the Cossacks into Paris, for an amenorrhœa, which was solely produced by the alarm she then suffered. And Dr Churchill states that almost all the women who are sent up to the Richmond Penitentiary, near Dublin, after having been tried at the Recorder's Court, labour under suppression, in consequence of the mental agitation and distress they have undergone.

" If it be asked how these causes operate, I reply, very differently. The effects being modified not only by the intensity of the cause, but in great measure also by the constitution of the individual.

" In a young or middle-aged woman, fleshy, of plethoric habit and ruddy complexion, the immediate suppression of the secretion will be followed by congestion, if not by inflammation. While in a woman delicate, thin, and spare, of sallow aspect, and highly nervous, the more probable consequences are irritation, attended by spasm and paroxysms of severe pain, with intervals of ease. In the former case, there will be sensations of weight and pain in the head and loins, tension and acute and constant pain in the region of the uterus aggravated on pressure, short breathing, a hot skin, and a full, hard, and rapid pulse; occasionally there will be violent hysteria, and I have several times observed delirium. It is scarcely necessary to add that suppression, accompanied by such symptoms, is far more immediately dangerous than any of the other derangements of menstruation. Gooch, indeed, mentions an instance of suppression, where, after death, the uterus was found in a state of gangrene, the result of intense inflammation.

" About this form of suppression, then, there can be no doubt;

neither the symptoms nor the treatment are at all masked or perplexing; it is much more than irritation; it is decided inflammation of the uterus. Apoplexy is said, by Capuron, to result from sudden suppression: this I have never met with, although I have seen seizures of a mixed kind, where there was something beyond hysteria, an approach to epilepsy. Partial and transient paralysis of the lower extremities has occurred once or twice in my practice; and Churchill mentions that the patient may also be attacked by local inflammation, either of the brain, lungs, or intestinal canal.

" The *pathology* of acute suppression is clear. There is, in the marked cases, inflammation of the substance and of the investing and lining membranes of the uterus. Of course, a similar remark is true of other viscera, when they are inflamed during menstrual suppression."—Pp. 66–68.

The treatment must be regulated by the effects of the suppression, immediate and remote. Blood-letting is most commonly indicated and can rarely be dispensed with. It may be general, or local according to the severity of the symptoms. Laxative medicine might also be conjoined; and unless one after first blood-letting the symptoms are alleviated, it may be requisite to employ a second. The warm bath at 96° or 98°, and local fomentations are also beneficial.

In the irritable and the hysterical, on the other hand, general bloodletting is inadmissible; nor is local bleeding by means of leeches always advantageous. A more eligible and suitable course is to employ aperient medicine, laxative and purgative enemata, the warm bath, the sinapised hip-bath or mustard pediluvia. Saline mixtures with camphor and antimonial powder are also recommended.

" Injections into the rectum sometimes produce an almost magical effect. Laudanum, assafœtida, and poppy syrup are employed for this purpose, (vide formulæ), and as it is necessary that they be retained for some time after their introduction, a piece of sponge or a napkin, should be kept firmly and closely applied to the extremity of the bowel. When narcotic enemata are injected, the quantity should not exceed two or three ounces, as more will unnecessarily dilute the anodyne ingredient, and by distending the gut, induce expulsive action. The pain and spasm, in this form of acute suppression, are thus relieved, and menstruation does oftener recur during the immediate period, in this, than in the inflammatory species; but in neither can it be invariably expected. If, however, the treatment has fortunately re-established the discharge, every precaution ought to be employed to prevent the exposure of so susceptible a patient to any of those causes which might induce a relapse. It need scarcely be remarked that an attack, either of inflammation or irritable suppression, is often the prelude to more permanent menstrual obstruction; and if month after month elapses, without the performance of the secretion, chronic suppression, to be next treated of, will be the result."—P. 70.

Chronic suppression is a much more tedious disease and more difficult to manage properly, and hence it may prove to be greatly more serious.

" The *pathology* of chronic snppression, where it does not depend on organic disease, may be referred to torpor or congestion in the earlier stages, and to coustitutional debility in its more advanced periods, In the protracted and inveterate forms of suppression, the ovaries and uterus, in common with other organs, suffer from defective nutrition, the blood having become too impoverished to excite the organic nerves, and to supply the requisite secretion to the several tissues of the body."—P. 72.

On the subject of Complicated Amenorrhœa the following notices are given.

" The six cases of complicated amenorrhœa were very interesting. In one, it was associated with chorea. This patient, after protracted treatment, was eventually cured by sulphate of zinc and the injection of *liq. ammoniæ* into the vagina. In another, amenorrhœa was complicated with epilepsy. The medicine prescribed was *ferri sulph., gr. i ; pulv. digitalis, gr. i ; pulv. myrrhæ, gr. ij ; mucil. Acaciæ, q. s. fiat pilula ter die sumenda.* It is worthy of remark that these pills were persevered in for three weeks, without any injurious consequences from the use of the digitalis, a circumstance attributable, probably, to its combination with the iron. At this period, the catamenia appeared, and there has been no return of the fits. In a third case, hemiplegia was attendant on the amenorrhœa. This complication was tedious and difficult to manage. At first, the *mist. ferri e.* was prescribed ; afterwards, the sulphate of zinc ; and an iodine liniment was well rubbed over the spine night and morning. Menstruation was eventually established, and the patient regained the entire use of the side. In the fourth case, there was tænia with the amenorrhœa. In addition to the other remedies, the *ol. terebinth.* was curatively employed. In the fifth patient, there was vicarious discharge from the mamma, in conjunction with amenorrhœa ; the *mist. ferri c.* was ordered, as well as the daily employment of the ammoniacal injection. The last patient had, in addition to the amenorrhœa, a peculiar nervous affection of one of her lower extremities, which completely subsided when the catamenial function was, by appropriate remedies, healthily established."—Pp. 73-74.

An ample account of emmenagogues follows. These the author distinguishes into two great orders ; Local or immediate Emmenagogues directly applied to the uterus and the neighbouring organs ; and Constitutional Emmenagogues, or those which produce their effect through the medium of the constitution. To the first order are referred electricity, galvanism, leeches applied to the *os* and *cervix uteri*, stimulant injections for instance liquor ammoniae in milk thrown into the vagina; stimulant injections into the uterus; medicated bougies; sinapised hip-baths; and other medicated baths; stimulating enemata into the rectum; leeches to the vulva, and frictions applied with stimulating embrocations over the hypogastric and lumbar regions.

To the second orderof emmenagogues or those operating through the medium of the constitution, Dr Ashwell refers mercury either in the form of blue pill, Plummer's pill, calomel, the gray powder, (*hydargyrus cum creta*), or the use of mercural ointment; iron and the chalybeate springs; spurred rye; iodine, strychnine, madder, rue, savine, seneka root, black hellebore, nitre, aloes, and similar irritating drugs.

Of all these remedies, which are numerous, very much in the ratio of their occasionally inefficiency, and sometimes the danger which their administration involves,—the safest and probably the best are iron, iodine, and aloes.

III. Vicarious menstruation consists in a discharge generally of blood from other parts than the uterus; superseding menstruation, and in its return, occasionally observing a menstrual period.

This vicarious discharge, the author thinks, can scarcely be regarded as a disease, when the hemorrhage does not really derange the system and exhaust its powers. It is an anomalous disorder. In general it takes place in females not married; but as often in the weak and delicate as in the robust. When it takes place in married females, it is generally associated with sterlity, a circumstance from which it is legitimate to infer that its antecedents and its presence are connected with a considerable change in the uterine circulation.

Though the vicarious discharge is usually blood, Dr Ashwell remarks, it may consist in an excess of the natural mucous secretion of the generative organs, constituting leucorrhœa. Upon the same principle we think the author should refer to this head, the secretion, and ejection of membranous substances. The discharge, when consisting of blood, may take place from the pulmonary or bronchial membrane, from the gastric-mucous membrane, the intestinal, the mammary, or even those of the eyes, mouth, and certain parts of the cutaneous surface, and, as is well known, from an open ulcer.

To the same head the author refers vicarious leucorrhœa.

" There can be no doubt that vicarious leucorrhœa, depends not alone on disordered action of the secretory apparatus of the uterus, but also on the impoverished and attenuated condition of the blood; and, in those instances where the discharge is always present, we may probably infer the existence of sub-acute inflammation of the uterine lining membrane. It is difficult to explain by what diseased action it is, that vessels accustomed to eliminate a sanguineous fluid should so far lose their full secretory power as to furnish only an increased amount of mucus. Some years ago I pointed out a similarly perverted action, occurring after labour, where aqueous discharge, occasionally in immense quantity, is poured forth instead of the lochia. And in hemorrhage from the intestines, an analogous phenomenon is sometimes presented, when a large quantity of mucus, exhaled from the villous coat, supersedes the sanguineous flow. Dewees regards vicarious leucorrhœa as a slow development of the menstrual function. Friend and Astruc were both cognizant of its occasional existence. The former denominated it 'the lymph-like menses,' and Astruc recognises it as leucorrhœa taking the place of the catamenia. Nauche, in his comprehensive, valuable work, 'Maladies propres aux Femmes,' looks upon vicarious leucorrhœa as salutary, thus confirming the views I have guardedly expressed. He says that in 1824 he had under his care a patient, twenty-four years old, plethoric and robust, but healthy sanguineous menstruation was absent. Instead of it, there was secreted every month, and with satisfactory results as regarded the health, a quantity of white mucus."—Pp. 97, 98.

IV. Of Dysmenorrhœa or Difficult Menstruation, accompanied with more or less pain and scantiness of the secretion, four forms are mentioned; the irritable or neuralgic; the plethoric; the congestive; and that from mechanical obstruction of the *os uteri.*

The first is the irritable or neuralgic Dysmenorrhœa or that in which pain is disproportionally acute, yet without indications either of inflammation or congestion in the parts. This we believe to be by far the most frequent form of the disease, and indeed to be that which constitutes the type or standard of the disorder.

The second variety is the *plethoric*, in which little deviation from the symptoms of the neuralgic form is observed; but the menstrual period is preceded by headach, flushing of the face, full quick pulse, a sense of weight in the pelvis, shiverings, and sometimes by delirium. These symptoms are followed by the catamenial discharge, sometimes profusely, with more or less coagula. In other instances which are said to be more frequent, the discharge is scanty, and consists of clots with shreds of membrane, all of which come away with extreme difficulty.

The third variety is the *congestive,* which is also attended with intense uterine pain, and a sensation as if a foreign body were confined within the uterus. There are, nevertheless, it is said, no inflammatory symptoms. In this form false membranes are expelled during the intervals of the attacks; and very uniformly an abundant leucorrhœal discharge contributes to exhaust the powers and health of the patient. It is in this variety that spurious abortion takes place in the ejection from the uterus, of a mass consisting of condensed or laminated coagulum, with portions of membrane, or a membrane moulded upon the cavity of the uterus. Such substances are delineated by Dr Grenville, and they are accurately described by Dr Montgomery.

The fourth variety, or that from mechanical obstruction, Dr Ashwell is disposed to admit as a substantive and real variety, upon the authority of the late Dr Mackintosh of this place. We cannot say that we hold very fixed or decided opinions on this point; but it appears, and always has appeared to us, a fair subject for inquiry, whether the alleged obstruction of the *os uteri* do not proceed from general tumefaction and congestion of the neck and orifice of the womb. Besides, it was not only of Dysmenorrhœa that this state was supposed to be the cause, but of Amenorrhœa, if we rightly understand the views of the proposer of this doctrine.

Much has of late been said upon Dysmenorrhœa as a disorder depending upon inflammation of the ovaries and uterus, chiefly the former; and we felt it requisite to state what we conceive must be regarded as valid objections against the general reception of this hypothesis. It cannot fail to be advantageous amidst the conflicting views which have been of late years brought forward upon this subject, to know what is thought by a practical observer like the present author.

"*Pathology.*—Some authors regard dysmenorrhœa as a neuralgic affection, a disease of enervation, (Innervation ?) identical with hysteralgia and irritable uterus. Others take a different view of the malady, and consider it as invariably dependent on a morbid condition of the mucous lining of the uterus. The former attribute the pain, and all the other symptoms, to acute irritation; the latter, to inflammatory action. These conflictiug opinions are not irreconcileable. In a great number of cases, particularly in delicate single females, there is only acute nervous irritation, affecting the muscular tissue as well as the mucous lining, and producing pain. In other and aggravated examples of the malady, there exists a low form of inflammation, modified by the peculiar membrane which it affects, and inducing intense pain. There are facts strengthening the impression, that dysmenorrhœa is often a disease of irritation. Hysterical and susceptible females are more prone to it than any other class. The pain is mostly unaccompanied by symptoms of inflammation; the pulse is quickened only during the paroxysm; this being over, it again subsides; and the flushed face and hot skin are often succeeded by a cold and clammy surface. Acute pain may exist without inflammation, and a layer of lymph may, as in dysentery, be thrown off from the intestinal mucous surface, without decided inflammatory action. Neither the pain of labour nor the after-pains, occasionally so protracted and agonizing, are inflammatory; and yet, if the degree of pain were to be the criterion, one might easily believe that inflammation really existed; for certainly no suffering from dysmenorrhœa can be greater than the pain attendant on labour and its subsequent contractions. In other cases, and which are thought (by the supporters of the opinion that inflammation is invariably present), alone to deserve the appellation of dysmenorrhœa, a modified and low form of inflammation certainly exists, which produces a false membrane, assuming, in some instances, the shape of the uterine cavity, and in others being expelled in detached portions. I quote the following remarks from a clear and practical paper on this malady, read before the Medical Society of Guy's Hospital in 1839, by Mr Henry Oldham, one of the obstetric assistants of the institution.

" ' There are so few instances in which minute investigations of the exact condition of the mucous membrane and body of the uterus in dysmenorrhœa are permitted, that we cannot offer any remarks on the visible changes in these structures. We are consequently left to the far less secure, but the only admissible mode of inferring the character of these alterations, from the symptoms which accompany the affection and the analogous productions from other mucous membranes. There can be no doubt that mucous membranes, though prone to suppurative inflammation, often throw off from their surface false membranes of organized lymph. The trachea in croup, the intestines in some forms of dysentery, the urethra under inflammation, are familiar examples of the kind; and there is but one common explanation of this occurrence, viz. that the products of inflammation are present, and that the lining membranes of these several organs are the seat of the diseased action. The uterus presents a precisely analogous instance in dysmenorrhœa, and we hence infer, that its mucous lining is in a similar state.'

" A paper presented to the Statistical Society in 1839, by Mr Lever, another of the obstetric assistants of the Hospital, and displaying his usual research, confirms these views, by showing that one of the occasional ultimate results of dysmenorrhœa, is structural change of the uterus. A protracted dysmenorrhœa, where the false membrane has been habitually secreted, and the fact will be noticed in the appended cases, is liable to induce thickening and induration of the *os* and *cervis*. This is the result of chronic inflammation, and is only to be feared from the probability of its assuming a malignant character in women possessing a cancerous diathesis. By such a structural change, whenever it affects the channel of the cervix, mechanical dysmenorrhœa may be induced."—Pp. 108–9.

It is manifest from what Dr Ashwell here says, that the reconciliation of the conflicting opinions consists not in admitting that there are at once irritation and inflammation in the same individuals and in the same class of cases, but that in a certain class which is allowed to be numerous, the process of menstruation, though very painful, is accompanied only with those symptoms believed to denote the presence of irritation; while in other instances which are probably greatly less numerous, these symptoms may proceed to a certain form and degree of inflammatory action. This we regard as an explanation properly speaking, not the reconciliation of contradictory opinions. After, however, having said so much on this subject, in speaking of the doctrines of Dr Tilt, in volume seventy-sixth, and subseqnently on the rational views propounded, in the excellent treatise by Dr Tyler Smith in the present volume, it must be unnecessary to recur to the subject; and we leave it, suggesting to our readers to compare the observations of Dr Tyler Smith with those of the present author.

On the important subject of treatment the author appears to great advantage. He distinguishes the therapeutic measures indicated, and to be employed, into two general heads, according as the leading symptoms predominate and indicate the presence of a mere neuralgic state, or one of inflammatory and congestive character. It is manifest that he regards the variety represented to proceed from, and depend upon, mechanical obstruction, as either so rare as not to require the use of mechanical dilating instruments, as a separate system of treatment, or as coming under the head of the second form, or as in certain instances altogether imaginary. At the risk of some repetition we subjoin the principal part of his therapeutic instructions.

"In the most ordinary variety of the malady, the *neuralgic*, the mustard hip-bath, and some narcotics, are especially beneficial; if the attack be aggravated, or of frequent recurrence, then ipecacuanha or antimony ought to be employed. But to be more precise; let the patient, on the first premonition of pain, commence the use of the hot-bath at 96° or 98°, and ordinarily remain in it for a half or three-quarters of an hour, repeating it three or four times in the twenty-four hours, and always guarding against the effects of cold, by keeping in a hot bed, so long at least as to allow the skin to resume its ordinary temperature. When the pain is very severe, the bath may be continued until faintness is induced; and if it be inadequate for this purpose, then half a grain of ipecacuanha, or fifteen or twenty drops of antimonial wine may be exhibited every hour. It will be found that the general relaxation of the cutaneous surface, and the attendant and temporary depression of the system, greatly favour menstrual secretion. In the milder cases, a hot hip-bath and slight narcotics will suffice. Camphor, ipecacuanha, and hyoscyamus, the Dover's powder, the extract of hop, lettuce, conium, and belladonna, variously combined (*vide formulæ*), relieve the pain, and induce perspiration and sleep. If perspiration is with difficulty obtained, three, or four, or five grains of antimonial powder may be given, by which a diaphoretic effect will be insured. Sometimes, when the pain is terribly severe, and the cerebral excitement goes on nearly to delirium, one quarter or half a grain of the acetate of morphia may be given at night, and smaller doses, either of it, the muriate or meconate of morphia, or Battley's *liquor opii sedativus*, at intervals during the day. A suppository of opium, conium, hyoscyamus, or belladonna, is often effectual in relieving the pain. In some cases, from the forcing, bearing down efforts, we can distinctly recognise the attempt on the part of the uterus to expel a clot. Here the ergot, either in decoction or tincture, may be repeatedly given, and an interval of marked ease, however brief, will occasionally follow its expulsion. It would be difficult to speak with precision of every modification of treatment; but enough has been said to enable the practitioner, while he maintains the principle, to vary the mode of management. In a French publication of the Society of Agriculture, Sciences, and Arts, Dr Patin recommends the acetate of ammonia for dysmenorrhœa, enjoining, however, this caution, that, as it diminishes the discharge, it must be used with reserve. He says, that, so far from being a stimulant, it is really a sedative remedy; and he regards it as applicable in any case, whether of dysmenorrhœa, profuse menstruation, or menorrhagia, if there be morbid excitement of the female genital system. This physician gives from forty to seventy drops three or four times daily, the only inconvenience being slight and transient giddiness. I have no experience of this remedy.

In the *inflammatory* and *congestive* forms of dysmenorrhœa, in addition to the remedies already enjoined, local depletion is peremptorily required; and there are cases where there is vascular fulness, in which a small general bleeding will be advantageous. Congestion almost invariably prevents secretion: the unloading of the vessels, therefore, aids the flow of the discharge. In the majority of instances, cupping on the loins, to the extent of eight, ten, or twelve ounces, or leeches will suffice; and, if necessary, the bleeding may easily be repeated. Leeches to the *os uteri* have been already mentioned, and, I think, that, when well applied, they are decidedly more beneficial than any other local depletion. Several times I have witnessed their superior efficacy in relieving the severe pain; in one instance, where the patient had been in the habit, for several years, of being occasionally cupped, the relief afforded by thus directly unloading the congested vessels themselves, exceeded, to use her own expression, any idea she could have formed. The speculum tube may be introduced into the vagina prior to their application; and if the cervix be brought fully into view, neither the vagina nor any other part than this portion of the congested viscus will be fixed on by the leeches. Their use should, as much as possible, be confined to married women, and a clever nurse should be taught to apply them. The hot-bath, as heretofore advised, sudorific, nauseating, and anodyne medicines may all be used. Hot poppy fomentations to the abdomen generally, and particularly to the hypogastric region, injections several times during the twenty-four hours of poppy and conium into the vagina, so as constantly to bathe the lower part of the uterus, will encourage the discharge and soothe the pain.

" The *second* part of the treatment comprises the management during the catamenial intervals. In the neuralgic and irritable form of the disease, the object is to improve the general health, principally by attention to the disordered state of the digestive organs, and by the avoidance of local excitements. I need not dwell on the importance of mild and cordial aperients, in preference to drastic and mercurial purgatives. An occasional alterative, such as the grey powder, or a moderate dose of blue pill, is required, but active and repeated purging must be injurious. Afterwards, the various preparations of iron, combinations of the mineral and vegetable tonics, omitting them when the bowels are disposed to be relaxed, a nutritious and unstimulating diet, pure air, a temporary residence at places celebrated for their chalybeate waters, and gentle but persevering exercise, are the most important indications. Dr Dewees, one of our ablest obstetric writers, regards the neuralgic form of dysmenorrhœa as analogous in its nature to chronic rheumatism, and strongly recommends, in the intervals of the disease, the volatile tincture of guaiacum, adding, that he has relieved many dysmenorrhœal patients by the antiphlogistic plan, but cured none. He begins with drachm doses of the tincture three times daily, increasing the quantity to three drachms three times daily in a glass of wine. His success has been great; mine, although I have often tried the guaiacum, has been inconsiderable.

"In the *inflammatory*, and especially in the more frequent *congestive* form of dysmenorrhœa, local depletion, saline aperients, and spare diet, are particularly necessary, nor should they be discontinued till the volume of the uterus is satisfactorily lessened. The hot hip-bath three times a week, and warm injections of the *fotus papaveris* twice a-day, will topically aid the former and more important remedies. Afterwards, mild tonics will complete the restoration of the patient. Bearing in mind what has been already said, of structural uterine change following chronic dysmenorrhœa, mercury must not be forgotten in the inveterate cases. In the preceding chapter, when treating of its deobstruent effects, I pointed out certain conditions as unfavourable to its use; but, where these indications are absent, there is no medicine so likely to prove efficacious. It may be employed in both the varieties, if their continuance is exceedingly prolonged, and the false membrane is habitually expelled; but certainly mercury is most beneficial where there is a thickened and indurated cervix, the result of chronic inflammation.

"The effect of the inunction of the iodine ointment on the neck of the uterus, where it is enlarged and hardened, is most satisfactory. This subject will, however, be more fully treated of in the chapter on hard tumours of the *os* and *cervix*."—Pp. 111–104.

V. *Menorrhagia*, which is defined to be Inordinate Menstruation, both as to the frequency of return and the amount of the secretion; the blood issuing in the majority of instances directly from the uterine arteries;—is distinguished into two forms; first, Profuse Menstruation, either as to frequency of return or amount of discharge, or both, without uterine bleeding; and, second, Profuse Menstruation, accompanied by direct loss of blood from the uterine arteries.

The latter, the author distinguishes into three varieties; *a*, Acute or active Menorrhagia, occurring in the plethoric and robust; *b*, Passive and chronic Menorrhagia, taking place chiefly in the delicate hysterical, and exhausted; and, *c*, Congestive Menorrhagia, generally met with at the middle or more advanced periods of life.

In the description of the symptoms attending the first variety of the second form, Dr Ashwell speaks of *spasmodic* menorrhagia in contradistinction to the inflammatory. In elucidation of the difference between the two the author observes.

" Where inflammation is present, there will be fixed pain in the uterine region ; a hot dry skin, and a frequent hard and full pulse. Where spasm prevails, the pain will not be constant; but having continued a longer or shorter time, and often most severely, it will subside, and, after an interval, again recur with throes resembling the pains of labour. The discharge, too, is equally variable, ceasing for short periods, during the pain, and returning when it subsides. The pulse, during the spasm, is contracted, irritable, and quick; afterwards it becomes softer and slower, giving proof by this rapid change, of a state of system, neither of inflammation nor debility, but of irritation. The progress, duration, and severity of these attacks are extremely variable."—Pp. 136, 137.

The distinction is important in reference to the method of treatment and the kind of remedies; and hence the following directions are given.

" In *spasmodic menorrhagia*, to which I have already referred, the pulse is irritable and quick, not hard and full. The system is not plethoric, nor the pain constant, (vide cases) ; but it subsides and again recurs. Here bleeding, nitre, and digitalis, fail to relieve; and recourse must be had to antispasmodics, anodynes, and occasionally to alteratives. It is not always easy at once to distinguish this form ; but if antiphlogistic means have been tried unsuccessfully, the patient will often be cured by remedies of a different class. Dr Gooch says, ' that a lady labouring under spasmodic menorrhagia went through the whole routine of antiphlogistic treatment without any benefit. I then gave her, he adds, one grain of ipecacuanha every hour ; in eight hours she became nauseated and sick, and the discharge immediately ceased. This state of nausea was kept up for a day or two, and the discharge did not recur. When you have a case of menorrhagia, attended with a quick and irritable pulse, the pain subsiding and recurring, you may be certain that it arises from spasm or irritation, and that it will be relieved by antispasmodic remedies. The two best are ipecacuanha taken into the stomach, and assafœtida, with opium injected into the rectum. A grain of ipecacuanha is to be taken every hour till nausea is produced; which state must be maintained for a day or two, by repeating the same dose as frequently as may suffice for this purpose; and quiet local irritation in the uterus by injections of assafœtida and opium, by the enema antispasmodicum, form 34, 35, 36, 37, or 39, to be found at pages 125 and 126. There is a very marked connection between the pain and the discharge; for if you can relieve the one, the other will cease.'

"Nor must the treatment in the interval be disregarded; in profuse menstruation, unattended by real uterine hemorrhage, allusion has been pointedly made to its extreme importance. If the bleeding and the antiphlogistic regimen, practised as a period approaches, be exchanged, after the disease subsides, for nutritious diet and wine, the malady will not only continue, but it will become aggravated, and the loss, during menstruation, may be so large as to excite considerable apprehension. Many patients protract the menorrhagia by such error, and, by repeated discharges, the passive form is induced. A nice distinction is necessary here; thus, several times when I have thought that the loss has depended on debility, and I have unsuccessfuly exhibited the ergot and tonics, I have gone back to the antiphlogistic plan, and have cured the patient. Nor let it be forgotten, that local depletion is sometimes most beneficial; especially in those cases where, in the absence of general plethora, there is local uterine fulness."—Pp. 138, 139.

The third variety, namely, the congestive, is one of no small importance, chiefly, because, not only is it liable to be confounded with hemorrhage from organic change taking place as it does at or after the middle period of life, but because it may be associated with one or other organic change, and in all instances it is a disorder which it is difficult to treat. As this form of the disorder appears not hitherto to have received the attention which it deserves; we cannot do better than allow the author to describe it in his own words.

"It continues long, occasionally for several years, (vide cases) and frequently in alarming excess. It is often preceded and followed by large watery and leucorrhœal discharges, and pain in the uterine and lumbar regions is a common accompaniment. Its sympathetic effects on the brain, lungs, and heart, are occasionally severe; and, where the disease has continued long, thereis a general coldness of hands and feet, a feeble and quick pulse, and an anxious pallid and sunken countenance. The alterations in the size and feel of the uterus, which form a part of the disease, cannot, at this period of life, be recognized, without some alarm. The malady is not confined to one class of women. The plethoric are not, as far as I know, more prone to it than the debilitated and irritable. I have rarely, if ever seen it, before thirty-eight or forty years of age; but I have several times met with modified attacks, independantly of organic complications, after menstruation might have been supposed to have ceased.

In the milder instances, the symptoms already described, terminate after a more or less protracted continuance in the entire cessation of the function; but in other and more protracted examples, the symptoms are so extreme, as to excite real apprehension. The recurrence of the bleedings is uncertain, although in general, a catamenial period will be partially observed. Occasionally the loss continues for many weeks or months, without any complete cessation; the only appreciable change consisting in a diminished flow; or the discharge becomes either aqueous or leucorrhœal, and perhaps slightly or offensively odorous. In many cases there will, at the expiration of a fortnight, or midway in the interval, be a peculiar bearing, acute pain in the lower part of the uterus. Several of my patients have noted this pain very accurately, and have correctly regarded it as indicative of a repetition of the menorrhagia. This symptom has occurred too often, to allow me to doubt, that it is in some way connected with the affection. Dr Churchill mentions that in most, if not all the cases he has seen, there was considerable dysuria, and that, in several, it was necessary for the patient to lie down before the bladder could be completely evacuated. Irritation about the neck of this viscus, extending along the urethra to its orifice, is common; but the dysuria, especially to the extent related above, I have rarely met with. Nor must it be forgotten that after these morbid occurrences have repeatedly taken place, and when every thought of pregnancy has been given up, conception has occurred. Such an event, for a time at least, and perhaps permanently, cures the affection. It is therefore important to bear in mind its possibility. I have known two examples of healthy pregnancy under these circumstances, after an interval of five or six years from the former accouchements. Head-ache, embarrassed respiration, bleeding from ths nose, sometimes excessive, dyspepsia, impaired appetite, and emaciation are frequent concomitants; after all, these various mischiefs may only be temporary."—Pp. 141–142.

The diagnosis is important, yet not in general difficult. The presence of corroding ulcer, cauliflower excrescence of the *os uteri*, ulcerated carcinoma of the cervix, and polypus may in general be determined by examination; and if these be absent, and there be no evidence of a polypus within the cavity of the uterus, or organic disease of the uterine lining membrane, then it may be inferred, that the hemorrhage depends upon congestion.

It appears that this is not in all instances a fatal disease. In general, the malady is cured after a long course of oscillations between hemorrhage and its cessation; or at least, as the catamenial function ceases, says Dr Ashwell, the bleedings also cease to return.

Treatment requires to be conducted with judgment, discrimination and caution. During the intervals of the hemorrhage, local bloodletting by means of cupping from the loins, or leeches applied to the anus or vulva, or even scarification of the *os uteri*, may be practised. Cold saline affusion and washing, cold water injections cautiously administered, rest on a firm, rather hard mattress, with the use of moderate unstimulating diet, regulation of the bowels, so as to avoid constipation and plethora; and afterwards, if weakness require, the cautious use of wine, comprehend the principal measures which ought to be employed.

Next comes Leucorrhœa or the White Mucous Discharge, which is considered under three forms; the Common Leucorrhœa, mostly acute; the inveterate and chronic Leucorrhoea; and symptomatic Leucorrhœa.

An ample and accurate history of these different forms of utero-vaginal discharge is given.

Generally speaking, whatever be the source of the discharge, whether the vaginal mucous membrane, the utero-vaginal membrane, the lacunæ and follicles, the *cervix* and the *glandulæ Nabothi*, the discharge depends on over-excitement of some kind, a sort of congestive and irritable, if not inflammatory state of the parts, and should accordingly be treated by means calculated to abate and allay this over-excitement and perverted action.

"If, under the idea of its being 'a weakness,' tonics and stimulating injections are early exhibited, the discharge will probably become more excessive, purulent or muco-purulent, thin and watery, or ichorous, and the constitutional effects more aggravated. If, on the contrary, antiphlogistic and soothing treatment be adopted, the morbid secretion will diminish in quantity, and the general derangement will disappear. In many instances the cure is quickly effected; but in some, even where proper treatment is early and fully pursued, it lasts long, proves very troublesome, and eventually passes into a chronic and inveterate state. It need scarcely be said that the discharges, in the varieties now pointed out, originate in increased action of the vessels of the different parts. Females, therefore, of plethoric habit, possessing more than ordinary vigour of constitution, are more liable to such attacks than those who are feeble or less strong. And as the former is not so numerous a class as the latter, it may be affirmed that leucorrhœa, attended by weakness, is the more common form. Yet it must be remembered that some females indulge in the pleasures of the table, and drink too freely of malt liquor, wine, or spirits. In many affections thus produced, the cares of a family or a business, or more frequently the disinclination to exercise, or eventually the want of strength sufficient to bear its inconveniences, almost compel a sedentary life. Hence they became corpulent, but not strong; a larger quantity of blood is generated, vessels scarcely to be seen before become visible; the pulse is full, the respiration is embarrassed on slight exertion, and the functions of the bowels and the kidneys are badly performed. In these examples, and they are not very uncommon, particularly about the middle period of life, menstruation often becomes profuse, and the leucorrhœa excessive. Great care is required in the treatment; for if these undue secretions are suddenly stopped, apoplexy, inflammation of the liver, of the stomach or bowels, may supervene, and the patient be quickly destroyed. In the section on treatment these circumstances will receive their full share of attention. In the various degrees now pointed out, leucorrhœa is a common sexual malady ; nor is it difficult to believe, from the complication of its causes, the susceptibility of women, the frequent neglect of all treatment and the injudicious management so often adopted, that every variety of case shall arise."—Pp. 163, 164.

On the subject of antecedents and causes, Dr Ashwell makes observations which ought not to be forgotten by any one who wishes to conduct treatment successfully.

"The *first* form, which I have denominated the *common leucorrhœa*, and, *which is more idiopathic* than the others, owes its origin, especially in delicate and strumous females, to causes inducing increased action, and sometimes inflammation in the secretory surfaces and glandular apparatus of the genital organs. These are, the application of cold or moisture, frequent excitement resulting in debility, from excessive sexual intercourse, abortions from which the patient has only imperfectly recovered, quickly recurring labours, puerperal hemorrhages, menorrhagia, profuse menstruation, and undue lactation. The irritation of a pessary, or of stimulant injections on the vaginal surface and the cervix uteri belong also to this class. There are other causes which act only *indirectly*, and these, with the agencies already pointed out, may lead, where the common mild, and more idiopathic disease is uncured, to the *chronic and aggravated form*. These operate through the medium of the nervous system and by sympathy· Thus in amenorrhœa, where the functional or organic nerves of the uterus are affected, a leucorrhœal secretion is frequently set up, not only from the vaginal but likewise from the uterine mucous surface. Intemperance in eating and drinking often induce derangement of the stomach, bowels, and liver ; by sympathy with these, morbid actions of the uterine system may be induced ; and leucorrhœa will often be the result. Nor must irritation of the spinal marrow be omitted in this enumeration. I lately saw an example where leucorrhœa might be traced to morbid affection of the chord in the sacral region, as evidenced by tenderness when these parts were pressed. Here there was an unusual degree of lumbar and sacral pain, much more than could be fairly attributed to the quantity or continuance of the discharge. In such, a cure is obtained by rest, leeches, and stimulating embrocations about the loins and sacrum. Still, I do not wish it to be understood that dorsal, lumbar, and inguinal pains are the concomitants of leucorrhœa of this kind alone. For I am quite aware, that these symptoms are attendants on cases of leucorrhœa, where there is no such remote and influential sympathy.

"*Symptomatic leucorrhœa*. as its name implies, being the consequence of other and distinct diseases, may be attributed to any causes which shall produce uterine or vaginal irritation. Amongst these must be mentioned relaxation, prolapsus, and the other displacements of the womb, polypi of various kinds, affecting the uterus, vagina, or urethra, hard or soft tumours of the reproductive organs, ascarides, a pessary, and other bodies intentionally introduced into the vagina."—Pp. 168–171.

All the forms of this disease, and especially that which is chronic, are perplexing to treat. All means that promise to stop the discharge may have been tried; and too often for a time, all are unavailing and inefficient. Astringent injections, sometimes vegetable, sometimes metallic, are the means which have been most commonly employed. Among the latter, nitrate of silver in solution has in general been found most successful. But it is well remarked by the author, that any one of the ordinary metallic or vegetable astringents in lotion or in injection, however good they may be, soon lose their effect. Hence, may be deduced the inference that it is best to vary the lotion or injection.

On the other hand, it should not be forgotten that, in attempting the cure of Leucorrhœa, attention to the state of the general health, and especially to the condition of the cutaneous and mucous surfaces, with attempts to improve them, are of indispensable importance. The due regulation of the functions of the alimentary canal by means of laxative and aperient medicine, cleansing the surface, and rendering uniform the circulation of the skin, by means first of the warm bath, then the tepid bath, and finally, the cold bath, cautiously administered, will tend more to remove the disease, and to keep it away, than almost all local measures however active and powerful.

Turpentine and cantharides, which act on the mucous surfaces, Dr Ashwell states he has given often advantageously and a good many times with curative effect. This is in imitation of the practice of Dr Dewees, who further begins the treatment by placing the patient on milk and vegetable diet, and, in some instances, causing her to lose blood.

For a class of cases, which we believe were quite similar, Dr Gooch had a practice of administering small doses, that is from one to two grains of ipecacuanha two or three times daily; while, at the same time, he recommended the constant use of flannel trowsers, and careful regulation of the diet. This practice, we have understood, was remarkably successful. It matters little whether ipecacuanha or tartrate of antimony be given at first, for either might do, in order to rectify the circulation of the intestinal mucous membrane, and abate the chronic inflammatory state of the utero-vaginal mucous membrane. One objection to the use of ipecacuanha is, that it tends rather to constipate the bowels than to relax them. But this may be obviated by combining with it a little rhubarb, aloes or scammony. It is indeed of paramount importance to cause the colon and rectum to be duly and regularly emptied, by whatever means this is accomplished. Dr Ashwell himself states, that he lately saw a case in which the discharge, after being for weeks excessive, was restrained by giving five or six doses of blue pill, followed by an aperient of salts and senna. The first motions were highly offensive and scybalous. Afterwards they became healthy; and with no other treatment than ablution and removal into pure, dry, mild air, and the adoption of good diet and exercise, by which the function of the skin was improved, the cure was completed.

This is exactly the result which it is reasonable to expect. The local discharge is of little moment, except as an indication and effect of the bad state of the functions of the skin and the mucous surfaces. Place the functions of these extensive membranes in a more natural and healthy state; put their circulation in the condition in which they ought to be; and the local discharge will soon subside and disappear. "Let healthy digestion be restored" says Dr Ashwell, "and the lucorrhœa will gradually disappear." (P. 181).

What is understood by good diet, it may be asked, in the treatment of this disease? Is the patient to have animal food daily or twice daily, as some persons interpret the phrase good diet, with an allowance of malt liquor or wine, or both. Dr Ashwell we perceive advises that animal food be taken twice daily, (p. 182) by patients affected by this disorder. But we cannot help thinking and saying, that this is not a safe course, at least in the commencement of treatment; and a proof that we are right, is found in the circumstance, that Dr Ashwell shortly afterwards allows, that it is not always safe to cure an inveterate leucorrhœa, without increasing for a time the action of the liver and the intestines, or putting the patient on a spare diet (p. 183). The truth is, that it is difficult, if not impossible to cure leucorrhœa, unless the diet is reduced and regulated, and the functions of the intestinal canal amended. Animal food should therefore be employed at first very sparingly, or altogether withheld, until some impression is made on the disease; and the safest sort of diet will consist in milk with grain in various forms, eggs, and similar articles. As to malt liquor or wine, to which he adds strong tea and coffee, Dr Ashwell allows that their use requires strict limitation, though he admits that in certain districts, and controlled by medical authority, their use may be beneficial.

"I recollect many years ago an old practitioner, in the fenny and damp part of Lincolnshire, who said that the disease was almost endemic in his neighbourhood during certain parts of the year, and that he combated, and often cured it, by bark, wine, gin, tea, and coffee. In Belgium and Holland, and round Berlin, the atmosphere is often loaded with moisture, and there it is common to attempt the cure of leucorrhœa, which is very prevalent, by spirituous liquors, tea, and flannel clothing."—Pp. 181, 182.

On the above evidence, as well as on the recommendation which follows, to use as drink hot water with a third or fourth part of brandy or rum, we remark that the whole of these stimulant spirituous liquours are most hurtful, and do no manner of good in promoting the cure of the disorder. When persons, who have taken gin-twist or brandy and water, get well of these complaints, the recovery has been effected not in consequence of the use of these articles, but in spite of them, and in opposition to their use; and the patients most certainly would have become better in shorter time without them. Such, however, is the effect of the vulgar error, which assigns to these liquors the name and character of support, and the power of increasing strength; such is the craving among invalids for them; and so deficient are physicians in fortitude, that while the former entreat to be allowed some of these articles, because they are so habitually taken, the latter have not the firmness and sense to refuse them, and the candour to explain to their patients that the latter are prolonging their sufferings, and increasing, rather than diminishing, the weakness of which they complain. Let this be altogether reformed.

The tenth chapter contains an elaborate and correct exposition of Hysteria. But our limits permit us not to enter upon the consideration of a subject so extensive and so complicated. We recommend the chapter to the attention of readers.

In the eleventh chapter is given an instructive account of that perplexing disorder, Irritable Uterus.

This disease, which consists in a permanent and painful sensibility of the uterus, especially its neck, Dr Ashwell shortly characterises as a constant Dysmenorrhœa. The disease usually takes place during the middle period of life, and is exceedingly difficult to cure, or even to mitigate in its symptoms. In general it is a disease of the married, though he allows that single females after thirty, or at the approach of the period of catamenial decline, and widows, may be its subjects.

The description of the state of the parts deserves attention.

"In this state the uterus is almost always a little lower in the pelvic cavity than natural, and by some authors it is said to be even constantly prolapsed. An external examination of the lower part of the abdomen gives pain; and if the finger be pressed behind the pubis and round the pelvic brim, a paroxysm is not unfrequently induced. But it is by an examination of the neck of the uterus that we discover the real nature of the malady. It cannot be said that the body of the organ is free from morbid tenderness; but the sensibility of the cervix is often so exquisite, that the patient shrieks and is thrown almost into a fit of hysteria if it be rudely touched: and on several occasions, by patients at Guy's, I have been requested to forego any renewed examination, because so many hours elapsed before the intense suffering subsided, even after the slightest pressure. Dr Gooch believed that the tenderness was confined to the uterus, and that the finger might be pressed against the sides of the vagina without causing uneasiness. My experience has not confirmed this view, having frequently observed a more than ordinary degree of sensibility and heat in this canal. In acute inflammation of the cervix uteri, a much less common disease, the observation is, I think, perfectly true; as then pressure on the vagina, immediately contiguous to the neck, or on the body of the uterus, is borne without any suffering at all. In irritable uterus, the cervix is often somewhat shortened and expanded, and occasionally puffy and swollen, and the lips of the arc more than naturally closed.

" Dewees especially remarks, that in all his cases a pulsating, throbbing, or fluttering sensation within the vagina or in some part of the pelvic cavity, always disagreeable, though not constantly present, and sometimes interrupting sleep, frequently occurred. Such symptoms have not existed in marked degree in the examples I have seen; but in several the throes of the uterus have been painfully severe."—Pp. 241, 242.

The facts now stated, would tend to create doubt, whether all this pain and suffering consisted merely in elevation of sensibility, without some change in organization if not change in structure. The symptoms now enumerated, would lead to the conclusion, that the vascular system of the neck at least of the womb was overloaded, the vessels distended, the blood prevented from moving through them; in short, that there was chronic congestion of the vessels of the *cervix* and vagina also, perhaps even of the body of the *uterus*. Accordingly, Dr Ashwell subsequently observes:—

" That the disease in question should be regarded as a modified inflammation of the cervix uteri, is a view, in accordance not only with symptoms, but with the results of the most successful treatment. It is difficult to understand, that there shall be redness, which I have several times seen by the speculum, heat, permanent pain, and tenderness of the neck of the uterus, a glandular part, without believing that its vascular and nervous structures, shall have undergone some change. Judging also from the marked relief afforded by cupping, leeching, aperients and spare diet, in Dr Gooch's own cases, what more tenable and satisfactory conclusion can be arrived at, than that the so-called irritable uterus is really dependent on subacute or chronic inflammation; a position, the truth of which is fully substantiated by those changes of structure, which, although slowly, and not till after many years, have nevertheless occurred, in cases, which till then, were regarded and treated as examples of irritable or neuralgic disease."—Pp. 243.

The whole of the subsequent observations are valuable, not only for the facts which they adduce, but for their tendency, and the inferences which they unavoidably and unambiguously suggest. We can, however, admit only the following as deserving attention in illustrating the true nature of the disease:—

" In a poor woman of the name of Turton, long under my care at Guy's Hospital, I had an excellent opportunity to test the truth of these opinions. She was an out-patient for three or four years, and during the whole period was suffering from irritable uterus. Constantly I pointed out her case to the pupils, as an excellent example of this affection. Numerous remedies were tried; and sometimes relief was obtained, but more frequently the various measures independently of narcotics, did little or no good. At length, after nearly six years, induration occurred. She became an in-patient, and the entry by the clinical clerk in September 1838, is as follows:—'The cervix is extensively destroyed by ulceration, and is also indurated."'—Pp 244.

Assuredly, it would not be legitimate reasoning to deduce conclusions from one case, or even from several. But when it is very constantly found that this irritable state of the cervix is connected with heat, swelling, some delapsus as it were, and that it is relieved, as we have several times seen,—by no means so effectually as by local depletion by means of leeches; it amounts as near as may be expected in pathological reasonings to proof, that, though perhaps there may be exalted innervation, yet this depends upon exalted vascular orgasm, upon congestion of the parts, and great derangement in their circulation.

In the treatment of this disorder, Dr Ashwell recommends that two indications must be observed, namely, to mitigate local suffering and to sustain and improve the general health.

At first he advises absolute repose in the horizontal position, not for weeks only, but for several, perhaps many months. The patient, however, should be carried into the open air, and if possible be placed on the sea coast. When exercise is attempted, it should be in the recumbent posture. The diet should be good, not stimulating; milk in every form in which it can be procured. Among drinks he allows claret and bitter beer. On the other hand, confinement within doors, low diet and aperients must be avoided.

As to local treatment, scarifications of the *os uteri* seem to be the only means of which he approves. Even much bleeding by leeches he regards as hurtful and to be avoided. Of the use of pessaries he speaks favourably. But they are articles which, we think, should not be used, unless it be found that it is impossible to do without them.

We have here completed the account of the First Part of the work of Dr Ashwell; and we trust that the sketch will enable readers to form their own opinion of its merits. We have had such frequent occasion to express an opinion in favourable terms, that it becomes unnecessary to add any thing upon that head. But it is right to inform readers, that they will find in this volume a large amount of excellent information, the value of which is in no small degree enhanced by the cases with which each subject is illustrated.

We defer to another [illegible] the examination of the Second Part of the Work. Meanwhile, we can assure readers that its merits are in no respect inferior to those of the First Part. The whole work is in short a Practical Treatise of great value.

ART. II.—1. ***Practical Observations on Mineral Waters and Baths; with Notices of some Continental Climates, and a Reprint, the third of the Cold Water Cure.*** By EDWIN LEE, Esq., Fellow of the Royal Medico-Chirurgical Society, &c. &c. London, 1846. Post 8vo. Pp. 134 and 42.

2. ***Bradshaw's Companion to the Continent. A Descriptive Handbook to the Chief Places of Resort; their characteristic Features, Climate, Scenery, and Remedial Resources; with Observations on the Influence of Climate and Travelling.*** By EDWIN LEE, Honorary and Corresponding Member of the Principal Continental Medical Academies and Societies. London, 1851. Folscap 8vo. Pp. 408.

3. ***Lectures on the German Mineral Waters, and on their Rational Employment for the cure of certain Chronic Diseases.*** By SIGISMUND SUTRO, M.D., Senior Physician to the German Hospital, Corresponding Member of the Physico-Medical Society of Erlangen, &c. London, 1851. 12mo. Pp. 431.

The mineral waters of the German States have long enjoyed, among the natives of those countries, considerable celebrity for the alleviation and removal of certain morbid conditions of the human body; and the physicians of the different countries of Germany have for a long series of years reposed considerable, it may be said, in some instances, a high degree of confidence, in the therapeutic powers of the different mineral springs in that extensive and varied country.

This confidence, however, has not in the same degree been shared by physicians in other countries. These gentlemen have been in all instances slow to admit, and in some instances have not admitted in the unqualified manner believed by the physicians of the German states, the therapeutic powers of the German mineral waters in the cure of disease. This disinclination was not wonderful, considering the circumstances under which the therapeutic employment of the German mineral waters became known.

During the continental wars of the first Napoleon, little was known of these numerous sources beyond the boundaries of Germany itself. To the physicians of Prussia, the Rhenish provinces, Saxony, Bohemia, and Austria, their effects and uses were in a certain degree known, as means of treating various slight chronic disorders, the result of indigestion and its effects. But to the physicians of other European countries, and especially of Great Britain, either the internal condition of Germany was altogether unknown, or any interest in knowing and studying it was absorbed in matters of sterner and more urgent importance. The state, also, of Germany in general, either in circumstances of actual war for several years, or groaning under the evils of foreign military occupation, was completely incompatible with the possibility of devoting much attention to the comparatively insignificant subject of mineral springs, either hot, cold, or indifferent.

At length in 1815 came peace, and the means of visiting continental countries, and knowing something of continental cities, was afforded to all, who possessed the two requisite conditions of time and pecuniary resources. The comparative tranquillity which followed, enabled the physicians of the German states to direct attention to a therapeutic method which had been to a certain extent popular, from the peculiar circumstances of the country, without being regulated by science, and which appeared in not few instances to restore health, after the pure aids of medicine had been exhausted. Numerous essays on the beneficial effects of different mineral waters appeared in the Medical Journals of the country, especially that of Hufeland and Osann, and monographies on the virtues of particular waters by different physicians, from time to time issued from the press. All the panegyrics, nevertheless, which these enthusiastic gentlemen could pronounce, did not contribute much to make the mineral springs of Germany be esteemed by physicians in this country. But, as usually happens, invalids, real or imaginary, from England, resorted to the waters, and either used them without advice, or by the advice of the local physicians; and it thus resulted that the reputation of these waters as therapeutic means was much higher and was more generally diffused among general and unprofessional members of society in this country than among physicians. This circumstance further continued rather to depreciate than advance with members of the medical profession, the character of the German mineral-springs; and many physicians in this country could see in them nothing beyond the usual effects of bathing in water hot or cold, or taking with regularity a certain proportion of a saline solution. All the benefits represented to proceed from the use of the German mineral wells were ascribed to the effects of good air, a dry climate, of scenery, of exercise, and all those influences which begin to operate, when invalids are withdrawn from the immediate scenes of their complaints.

The current, nevertheless, had set in pretty strongly upon the mineral waters of the continent. Facilities of access were afforded by the steam-vessels and railways. The writings of Dr James Johnson, Dr Grenville, and afterwards Mr Lee, tended to make the different springs more fully known to English visitors. The scepticism as to the actual utility of the waters, as mere waters, still continued, and probably it will long continue, among professional persons; whereas the unprofessional public repose in these waters a very considerable degree of confidence. It is not improbable, nevertheless, that, while by the Germans and the public at large, these waters are as therapeutic agents a little overrated, their true powers are either imperfectly understood or somewhat underrated by English physicians. Many of the German writers, whom it is unnecessary particularly to name, have employed, in describing the effects of different baths, language and encomiums so extravagant, that it is next to impossible for a sober mind to give any credit to the statements. Others have been perhaps more moderate in their eulogiums. But they have shown little discrimination in applying the different waters and baths to the morbid states of the system to which they are best adapted.

Meanwhile, upon the continent, both in Germany and in France, the knowledge of mineral springs and the study of their operation on the human frame, has grown by progressive degrees into a science by itself. Since the publication of the analyses of Berzelius many years ago, numerous expert and skilful chemists have added greatly to our knowledge of the chemical constitution of these waters. Many new ingredients have been discovered, where they were not suspected to exist; as iodine, bromine, arsenic, besides glairine, and baregine, and the crenic acid. More attention also, has been given to distinguish the exact forms of disease to which each spring is best suited; and it must be allowed, that the amount of knowledge upon the history and uses of these waters, is at once more ample and more accurate than it previously was. In these circumstances, it appears to us, that what is at present most wanted, is a full, impartial, and faithful account of the effects of the mineral waters of Germany, and a fair and patient hearing to those who undertake to expound the properties, alleged and actual, of these waters. It is principally amidst discussion and inquiry that truth is elicited.

To answer the purpose now mentioned, the lectures of Dr Sutro appear to be well calculated. The author evidently has taken great pains properly to understand his subject. His information bears all the marks of being correct and faithful. He cannot be accused of being an indiscriminate advocate for the use of these waters in all disorders; neither is he deficient in confidence in their operation and powers, when such confidence appears to be well founded. His history of the German mineral waters is minute, accurate, and scientific. His book is at once instructive and interesting.

To some of the leading subjects it may be serviceable at present to advert.

Dr Sutro adopts with Vetter the general division of springs into *Krenæ* or cold springs having a temperature below 15° Reaumur, or 66° Fahrenheit; *Thermae* or hot springs, including all those, the temperature of which rises above 28° of Reaumur, or 95° Fahrenheit; and *Pegae*, or those of intermediate temperature, that is, between 15° and 28° of Reaumur, or 66° and 95° of Fahrenheit. These terms, it is needless to mention, are adopted from the Greek; but it is proper to add, that while the German physicians use the original word Κρηνη to signify merely, a fountain, well, or source, they annex, we suspect rather arbitrarily, to the term Πηγη, the meaning of a spring or salient and forcibly gushing water.

Other distinctive denominations are *Halopegae* for the saline springs, *Chalybokrenæ* or chalybeate springs, *Theiokrenæ*, or cold sulphur springs, and *Theiothermæ* or hot sulphur springs, *Pickropegae* or bitter springs.

Topographical situation and relative altitude above the level of the sea are circumstances which exert some influence on springs, and which the author thinks must modify their medicinal efficacy. It has been calculated that the warmth of the air diminishes by one degree of Reaumur for every 500 feet in height. But when it is remembered that temperature is modified by the direction of mountain ranges and similar circumstances, it will be seen that elevation alone can have no great effect.

"Some of the Salzburg and Tyrol mountains approach Mount Blanc and Monte Rosa in height, the Gross Glockner rising to upwards of 11,000 feet; and the mineral springs of the upper valley of the Inn (Upper Engaddin) at St Mortiz, are situated 4000 feet above the sea. Several in the lower valley near Inspruck only lie 1700 feet high.

"The higher or lower situation of the *Bavarian* springs depends on their vicinity to the Southern Alps, the Bohemian Forest, the Fichtelgebirge and the adjoining Franconian mountains, or to the river districts of the Danube and Main.

"The same relation takes place with the spas of *Austria*, their altitude being determined by the neighbouring mountains of Styria and Carinthia, or by the Danube. If the course of the river be pursued upwards, you will perceive the proportionate height of the adjoining springs from that of the following terms:—*Vienna* is situated nearly 500′ above the level of the sea; *Passau*, nearly 800′; *Ratisbon*, 1100′; *Augsburg* on the Lech, 1400′; *Elm* on the Danube, about 1100′; *Munich* on the Iser, 1600′; *Memmingen* on the Iller, south of Ulm, nearly 1900′. In the whole of this river district of the Danube and Inn no spring is situated lower than 500′; several, on the contrary, much higher. You observe the range of fall to be 1300′ between Memmingen and Vienna.

"In *Bohemia* those near the Bohemian, Fichtel, Erz, and Riesengebirge are higher; those situated more in the centre of the kingdom, lower; *Prague*, the capital occupying nearly a central position, lies about 500′ above the sea, the surrounding mountains being from 2000 to 3000 feet high.

"In *Saxony* and *Thuringia* those springs belonging to the river domains of the *Elbe*, or to the fertile plains intersected by the river *Unstrut*, do not lie higher than 300′, some even lower; while those approaching the Thuringian Forest, and Erzgebirge, are much higher. *Dresden* on the Elbe, lies only 280′ above the sea; *Meissen* on the Elbe, about 200′; Leipsig on the Elster, 300′; whilst nearer the Thuringian Forest, *Erfurt*, to the west of tho Saale, lies nearly 600′ high; Weimar, to the north-east of Erfurt, 650′; Gotha, to the west of Weimar, 900′.

"The springs of *Wurtemberg*, *Baden*, and the *Rhine* are determined, as regards their altitude, partly by the vicinity of the Black Forest, Taunus, Eifel, or other mountain-ranges, bordering on the Rhine, and partly by their more northern or southern position. The level of the Rhine is, near Basel 765′ above the sea, near Mainz, 256′, near *Coblenz*, 190′, near Bonn 138′, near Cologne, 112′; whilst the highest peak of the *Black Forest* rise to upwards of 4000′, and of the Taunus to 2600′.

"Most of the Silesian springs are distinguished by an elevated position, being situated near the highest points of the county of Glaiz and the Riesengebirge. The height of the *Schneekoppe* is nearly 5000′, that of *Breslau*, on the Oder, only 400′. The spas to the south of Breslau, near the mountains, range in their altitude between 800′ and 1700′.

"Among the springs of *Electoral Hesse* and *Westphalia* those only claim greater altitude which arise near the Westerwald, the Röhn, the Haar, the Teutoburg forest, and the Weser mountains. The summit of the Rhön, at the south-eastern boundary of Fulda, rises to nearly 3000. *Fulda*, itself on the Fulda, lies about 800′ above the sea; *Cassel*, on the Fulda, more northerly, about 500′: *Giessen*, on the Lahn, about 400′; *Hanover*, on the Leine, only 240′. The mineral springs of *Pyrmont* and *Hofgeismar* between 400′ and 500.

"Near the shores of the North and Baltic Seas, in the absence of mountain-ranges of importance, the mineral springs are found to be comparatively in low situations, becoming of course lower the more they approach the sea.

"But a high position must not be considered as invariably rendering the air too keen or bracing. Thus *Ischl*, in Austria, to the east of Salzburg, situated in a highly mountainous region, at an altitude of 1400′, enjoys a very mild and temperate climate. The same climate obtains at *Pfeffers*, in Switzerland, near the Rhine, upwards of 2000′ high, and at *Leuk*, on the Rhone, upwards of 4000′ over the sea."—Pp. 21–23.

Mineral springs usually bear a relation, which may be more or less distinctly traced to the geological characters of the country in which they rise. Thus, though it cannot be always said that thermal springs constantly issue from volcanic rocks, yet these springs are always more frequent in countries of this character than those not so characterized. The Eifel and Taunus mountains are generally allowed by geologists to be of volcanic origin, containing trachyte, basalt, and similar allied rocks. Now in the former arise the springs of Berthrich on the west of the Moselle; and in the springs of Ems on the Lahn, and Wiesbaden on the east of the Rhine, who ever examines with a geological eye the Heyrich or Taunus range, and considers the site of Wiesbaden, will see that the latter district forms as it were the centre of a crater, and that the hot springs are probably the outlets of long chimneys, arising from a considerable depth in the interior of the globe.

Dr Sutro represents sulphurous and chalybeate springs as mostly independent of the general character of a region, and as probably isolated products of local causes; whereas acidulous and thermal springs can in general be traced to certain characteristic mountain chains. Thermal springs also, as a general principle, are supposed to proceed from deep lying strata; whereas cold springs issue in general from those which are superficial. Of the former, the Wiesbaden springs are good examples; whereas the neighbouring cold springs of Geilnau, Fachingen, and Selters, proceeding from the upper lime strata, containing slate, are believed to exemplify the latter proposition.

Saline springs are supposed to derive their origin from strata of rock salt, through which they pass, and which belong also to late formations, being frequently surrounded by lime, gypsum, and clay. In some instances it is not improbable that these are outlets of considerable subterannean brine—salt springs, rivers, or lakes; but as these must be supposed to derive their saline impregnation from beds of rock salt, it refers itself to the same general principle.

The saline springs of the German states may in general be traced in the direction of the great deposits of salts. The rich salt mines of Austria supply the brine springs of Ischl, Reichenhall, Rosenheim, on the west. The salt deposits near the banks of the Rhine, from Basel to Aix-la-Chapelle, supply the saline springs of Baden-Baden, Kreuznach, Wiesbaden, Soden, Nauheim, Schwalheim, Salzhausen, Burtscheide, &c.

In lecture third Dr Sutro considers the general chemical constitution of mineral waters as agents acting on the animal economy. The characters, which constitute and distinguish mineral waters, he observes are their peculiar composition and physical properties of taste, colour, temperature, and specific gravity; and above all their specific effects on the human organism. Water absolutely pure and free from saline impregnation, seems to be rare. The water of several wells used for drinking in Berlin, Hanau, and other towns contain from five to ten grains of solid ingredients dissolved in every sixteen ounces, yet without being in consequence considered as mineral springs. On the contrary the spa of Wildbad in the Black Forest, contains only three grains and a half in sixteen ounces; Teplitz, four and three-fourths; Gastein and Ppeffers, two grains and a half; Bruchenau, two and three-fifths; Spa, three and three-tenths; the sulphur springs of Landeck, two and six-tenths. Waters containing chlorides and sulphates possess most ingredients.

The author next directs attention to what may be considered the permanency in constitution of mineral-waters, and the peculiar properties of their high temperature, especially upon the human body, which is stated to be invariably corroborating, when ordinary baths of hot water are debilitating. But from these points we must pass to those most immediately medical. The following observations show the usual effects of hot mineral-waters upon the human frame.

> " An extremely hot bath will induce at its first contact a passing sensation of *chilliness;* a very cold bath of burning. How is this to be explained ? I think in this way: the first *shock* of high temperature produces a sudden contraction of cutaneous pores, for it drives the water vapour surrounding the body, the result of inherent heat, as we have seen above, towards the interior, at its onset, till violent reaction ensues.
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> " The first result of strong cold, however, is to cause a sudden escape of the excessive inherent heat in question, and thus momentaneous opening of the exhaling vessels, with a momentary sensation of burning.
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> " The effects of baths on the epidermis are in accordance with the horny character of the latter. Heat extends and swells it, and renders it more pliable; cold contracts it, and makes it more rigid. The highest and lowest temperature make it impenetrable for the exhaling substances, which therefore raises the cuticle in blisters.
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> " The highest heat borne without injury is 36° Reaumur, = 113° Fahrenheit. The normal excess of caloric is not only prevented from further development, but a part of the hot medium is communicated to it; therefore, besides the local sensation of heat, general immoderate reaction ensues, circulation becomes increased, the central nervous system agitated, and particularly arterial functions heightened. These efforts may be required as curative in certain morbid affections, when the nervous centres are to be brought into action from their peripheric ends. By the softening of the horny substance, the excretory canals are contracted and closed at their external ends, and thus the subcutaneous watery vapour collects and causes tension of the epidermis, which becomes fragile in consequence, and desquamates sooner or later. The violent irritation may easily produce febrile action. Thus chilliness may arise with other characteristic phenomena.
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> " Such elevated temperature may, therefore, be admissible in very few cases of general torpor, in certain spasmodic diseases; in

fact, when a curative reaction is to be expected, or when this general external derivation is to be safely employed, to restore diminished cutaneous innervation. But they must be considered *as extremes* in extreme states; at least for the whole body; for in such general torpor some internal organ is mostly in a state of irritation, forbidding the employment of such a violent remedy; but in the shape of partial baths, they may often be employed as excellent local derivatives.

" *Hot mineral waters* create a somewhat less sensation of heat, because part of the caloric is required to keep the dissolved ingredients in a liquid state. This is particularly the case in mud-baths, in the so-called chalico (earthy) and theia (sulphurous) thermæ; less so in the alkaline or saline thermæ. In the latter, the irritation of heat is supplanted by the chemical stimulus of soda or chlorine, corresponding with the other stimulus at least in its secondary result.

" *Baths of a temperature* between 20° and 30° Reaumur (77° and 100° Fahrenheit) would not produce any influence on the temperature of the organism if the animal heat had not the function of furnishing a regulating excess to the surrounding media. In some individuals this excess is very small, and would be withdrawn by a bath of 28° Reaumur. In orhers, again, of more robust and plethoric habit, this heat would produce effects described as appertaining to baths of 30° and 36° Reaumur.

In the lukewarm and cool bath, the warmth acts no longer as a stimulus inherent to the water. But the absorption of internal heat must influence those organs which perform the function of calefaction. The amount of heat which each atom of water requires to put itself into equilibrium with the heat of the body, and the rising of the warmed layers from the surface of the body upwards must materially lower the amount of animal heat, and primarily depress the external innervation; but the consequent reaction induces a powerful impulse from the interior to the circumference, tending to an increased production of heat, and a heightened metamorphosis on the surface.

" If the individual leaves a cold bath, the reaction produces such an excess of warmth as to guarantee a certain stability of external innervation and resistance to the changing temperature of the air (unless it be violently agitated). The air itself being a bad conductor, and requiring little latent heat for maintaining its gaseous form, does not deprive us of a great amount of warmth as already shown. The epidermic redness produced by a cold bath certainly resembles the visible effect of a hot one. But whilst the consequence of a hot bath is reaction to the interior, redness with congestive fulness of the epidermis, being the commencement of the phenomena, in a cold bath, external redness and increased circulation are the closing phenomena resulting from a reaction issuing out of the centre towards the exterior. The warm bath acts as a sudden excitant of functions, the cold as a sudden retarding influence. The individual organic resistance will decide whether the reaction will have a local or general character.

"Suppose, for instance, the urinary organs to be in an irritated condition, a sudden immersion into a cold bath, which momentarily prevents perspiration, will produce increased urinary secretion, or spasms of the secreting organs. If mucous membranes are abnormaliy affected, over-stimulation and great irritation may ensue through the sudden action of cold. The functions of the muscles sometimes become insensible and immovable through this diminished innervation.

"When increased calefaction takes place by some irritation of the peripheric nervous system, a cold bath will absorb this excess of heat, and violent reaction need not be apprehended, from the energy of the nervous system having been previously weakened through the over-stimulation. The cold bath will only be beneficial when its effects assume the type of fever—cold, warmth, increased secretions, &c.

"The duration of the cold bath is of great importance. After the first effects of weakened innervation have been counteracted by reaction, the steady-heat absorbing-power begins to act. The general secondary reaction, beginning with rigors, will appear, sooner or later, according to the individual power of resistance.

"Smaller quantities of water rapidly exchange their temperature for that of the blood. The evaporation consequent on the liquid applied always binds a certain amount of heat. Moist bodies under thick covering stimulate the cutaneous system to a greatly increased serous secretion. They act like a local vapour-bath, with this modification, that the body itself supplies the necessary warmth. Such moist applications and fomentations may be reckoned as powerful sudorifics, and might be very beneficially employed with sea or saline water in complaints characterised or produced by suppressed or retained perspiration, and by heightened nervous susceptibility to external influences.

"*Vapours* can be employed in much higher temperature than water, being worse conductors, and possessing a smaller capacity for caloric. But you may ask, 'Why should they be better borne than air?' The answer is very simple; because they require a great amount of heat to be kept in the gaseous form, which is not the case with the permanent gaseous mixture composing our atmosphere. They also less impede evaporation, in fact, promoting it. They are very useful where the liquefying process is to be heightened, where perspiration has been retarded, where noxious matters are to be excreted by the skin."—Pp. 47–50.

Besides the distinctions above mentioned, Vetter divides all mineral waters, principally in reference to their action on the human body, into two large classes; the *Akratopegæ* [α, κϱατος, πηγη] or those springs which have constituents, as it were powerless, though in efficacy they may be considerable; and *Synkratopegæ*, [συν κϱατος πηγη] or those mineral waters in which the power of the constituents bears a certain proportion to the observed effect.

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ring animal evaporation is to us an expression unintelligible.

Of the chemically indifferent thermal springs, Wildbad in the Black Forest, in latitude 49°, longitude 8°, is the first to which the author directs attention. It is a savage wild place, situate, about 1300 feet above the level of the sea, in a valley, around which the mountains rise to another 1500 feet. The surrounding mountains consist of ferruginous red sandstone and granite. The springs flow out of the clefts of granite rocks, gray in colour, granular in character, in four divisions from north to south, forming several independent basins. The springs vary in temperature from 25½ R.=88 Fahrenheit to 30° R. = 99 Fahrienheit. The waters contain chiefly chloride of sodium, carbonate of soda and lime, with some sulphate of soda, with iron, manganesium and potassium in very small quantity, and a large proportion of nitrogen.

It can scarcely be the chemical ingredients which produce the beneficial effects resulting from the use of the Wildbad springs, though the author ascribes part of the utility to the chloride of sodium which they contain. This explanation, nevertheless, is scarcely intelligible, as the quantity of this salt is very small.

The following explanation may therefore be more entitled to attention.

" If we see the use of a mineral water, causing distinct retrogression of these anti-vital phenomena; if we perceive gouty concretions to proceed towards absorption; if we observe contracted limbs gradually to relax again, and to try feeble efforts of long-forgotten exercise; if we find cutaneous harshness and rigidity to diminish, and to give way to a former softness; if we behold a resuscitated desire for muscular exertion and for mental work in a prostrate individual, and we know the spa, the originator of these changes, to possess a great quantity of azote, is it not legitimate to attribute to this gas part of the efficacy?

"Whilst the chloride of sodium exercises its well-known beneficial influence on organic metamorphosis, stimulating digestion by forming hydrochloric acid on the one hand, and combining with albumen as soda on the other, counteracting earthy formations, azote may powerfully assist this process, and contribute towards the curative changes."—P. 69.

The disorders for the cure or alleviation of which this spring is most celebrated, are gout, rheumatism, especially chronic, with contraction of the joints, arthritic paralysis, and contractions from wounds. The primary effects are to increase pain. But afterwards these pains are abated and disappear; swellings especially articular are dispersed; and the joints become moveable as the patient recovers health and strength. The reputation of Wildbad stands high with the Germans for removing the lameness and decrepitude of chronic rheumatism and nodous gout.—P. 111.

Of Pfeffers or Pfeffers a similar acrato-thermal spring, the author gives the following characters.

"The Spa showed itself efficacious, according to Dr Kaiser, in dyspepsia, flatulence, acidity and mucosity of the stomach, especially in *gastric cramps,* through repression of piles, of exanthemata, or of gout, in habitual diarrhœa, or obstruction, in abdominal plethora, &c.

"*Next, in disease of the nervous system :* hypochondriasis and hysteria, if based on suppressed action of the ganglionic system; if the organs below the diaphragm are impeded in their oxydation and decarbonization by arterial blood, to the disturbance of the vegetative nervous sphere, the diluted warm liquid penetrates to them, and induces a more vigorous transformation, and subsequently, a restoration of the previous equilibrium.

"*Metastatic gout,* particularly in nervous, irritable subjects, and neuralgic pains of the face, are relieved here: also abnormal catemenial flow becomes regulated, &c. &c.

But, as I stated above, the chief reputation of the Spa is in *paralysis,* contractions, and local weakness from wounds; but there must be no residue of apoplexia or congestion to the brain, else the Spa would be injurious. The paralyzed part must not be too emaciated, nor quite insensible, else the so-called 'Ausbade kur' might be required, which consists in persons remaining longer and longer in the baths every day, till several hours' stay, for a consecutive series of days, has produced voilent reaction."—P. 85.

If the alleged sanative efforts of this water be compared with its chemical constitution, the difficulty of explaining them, considering those of Wildbad, is not diminished. The leading ingredients are said to be carbonate of magnesia, and of lime with sulphate of soda. The temperature before cooling is 29 R. ≐ 97¼ F. and at Ragaz 27° to 28° R. = 92¾° — 95° F. The prolonged use of the bath causes what is called the bath fever and eruption, which in general assumes the appearance of the miliary redness and vesicles.

The seventh lecture on Ischl and Teplitz commences with an account of the descent to, and visit of, the salt mines of Salzburg.

Ischl itself lies in a valley at the foot of the Noric Alps, intersected by the rivers Traun and Ischl, the former flowing on the right to the Traun Lake through which it passes. The water of Ischl itself is rather pure; but it is rendered saline by brine taken from the salt mines. This brine contains a large proportion of chloride of sodium, with very small quantities of chloride of magnesium, sulphate of soda, sulphate of magnesia, and sulphate of lime, and chloride of calcium in the order now mentioned.

From Ischl, the author proceeds by an immense step to Teplitz, among the Erzgebirge mountains, in latitude 51°, east longitude, 14°. The springs here contain in small quantity carbonate of soda, and, in still smaller proportion, the carbonates of lime, manganese, magnesia, iron, lithia, and strontia, with chloride of silicum and sulphate of potash, iodine, silica, phosphate of alumina, and crenic acid. The whole do not amount to quite five grains of solid matter in sixteen ounces of water, Free carbonic acid and nitrogen are also present. Temperature, 38½ R.—to 39½, = 118–120, Fahr.

It is proper, here, to explain that Crenic Acid, that is, Fountain or Spring Acid (*κϱηνη*), is an acid principle which has been found in several springs, and which is supposed to be a product of lixiviated humus or geine. It imparts a yellowish colour to the water during evaporation. It is separated by treating the carbonates with acetic acid. If acetate of copper be then added, the Crenic acid will expel the acetic, and be precipitated as Crenate of Copper, in greenish-white flakes, in the proportion of 93·19 of oxide of copper, and 6·87 of Crenic Acid. It is not exactly known in what form Crenic acid exists in the springs, but it is probable that it is in combination or mixture rather with some of the carbonates.

The disorders in which the Teplitz springs mostly show their efficacy, are, chronic rheumatism, anomalous gout with articular swellings, and contractions, palsy after wounds and fever, and paralytic disorders ensuing upon metastatic eruptions.

In section eighth, the author examines the history of the *Pikropegæ*, or bitter mineral waters, distinguished in general by the predominant presence of sulphate ef magnesia (Epsom salts), or sulphate of soda (Glauber's salts), or both.

The *Pikropegæ*, in general, originate in volcanic mountains, or in strata of marl, and sometimes in secondary layers of gypsum.

Under this head the author considers the springs of Seidschutz, Carlsbad, Marienbad, and Franzenbad. These springs differ, nevertheless, in the proportion of their respective ingredients.

Thus Seidschutz contains a large proportion of sulphate of magnesia, nitrate of magnesia, and sulphate of soda, a good deal of sulphate of lime, a little carbonate of magnesia, and a small proportion of chloride and crenate of magnesia. Seidlitz, again, which is only half an hour's walk to the north-east of Seidschutz, contains no sulphate of soda, but an amount of sulphate of magnesia one-fifth greater than Seidschutz, and the other saline ingredients, but no nitrate of magnesia.

The Püllna spring, another of these bitter fountains, is remarkable for containing the largest proportion of sulphate of soda (123 grains in 16 ounces), a large quantity of Epsom salt (93 grs.), some sulphate of potass, and the other salts, but no nitrate of magnesia, and no iodide of magnesium. The Püllna water is believed to be more powerful as an aperient, a solvent, and a derivative agent, than either the Seidschutz or the Seidlitz spring.

Carlsbad, on the other hand, though placed among the bitter saline springs, is by no means properly entitled to that character. It is, doubtless, a saline spring, as it contains in the sixteen ounces nearly 20 grains of glauber salt (sulphate of soda). But it has a strong alkaline impregnation containing 9 grains and 7-10ths nearly of carbonate of soda; and to this with the carbonates of lime and magnesia it no doubt owes much of its power as an antacid in gouty concretions and rheumatic disorders. The combination of iron with these articles, and the large amount of carbonic acid are also conceived to be beneficial, the latter rendering the earths and alkalis more endurable, and the former metal acting as a tonic, and counter-acting any debility which might follow the alkalization of the system.

Besides its power as a remedial agent in gout, especially anomalous gout, the uric acid diathesis, and rheumatic gout,—all disorders in which acid predominates, it is said to have the highest reputation, and it is hoped this is well grounded, in hepatic and splenic *physconia*, the consequence of ague, and in colic produced by the presence of gall-stones.

Dr Sutro draws a vivid picture of the peculiar effects of the use of the Carlsbad springs. The purging produced by them he states differs from ordinary purgation by very fetid, bilious, mucous, and fatty discharges, more loaded with actual pseudo-formations or abnormal concretions, than with the serum of the blood. They are, therefore, he argues, more alterative of sanguification than weakening agents.

The choice of the springs, he represents, to be not a matter of indifference. If it be wished to produce a deeply penetrating action, and to excite the function of the skin and the kidneys, the hottest, namely the Sprudel, 59° R. = 141° Fahr. should be prescribed. If heightened alvine evacuation be the chief requisite, the Muhlbrunnen 44¾° R. = 132¼° F., or the Theresienbrunnen 42° R. = 126° F. is the proper remedy. But if the object be merely an alterative action and the means of subduing vascular erethism, the spring indicated is the coolest, namely the Schlossbrunnen 36° R. = 113° Fahrenheit.

The Carlsbad salt, the chrystallized salt of the springs is sometimes added to the waters, and sometimes employed alone in solution. It consists chiefly of Glauber's salt (sulphate of soda) with an inconsiderable proportion of carbonate of soda, and a small quantity of lithia.

Marienbad and Franzensbad come last among the catalogue of springs referred to the order of *Pikrokrenæ.* Both contain principally Glauber's salt, common salt, and carbonate of soda; but the springs of Franzensbad in smaller proportion than those of Marienbad. The waters of both sets of springs are employed nearly in the same manner and for the treatment of the same morbid states as those of the latter fountains in general.

Time and limited space, however, warn us that here for the present we must make a pause. In so doing, we recommend the work of Dr Sutro to all physicians who wish to understand the composition and nature of the German mineral springs, and the morbid conditions of the system in which they are likely to be beneficial. To patients and invalids it will not be necessary to say anything, except that they should not attempt the use of these springs, without obtaining previously the advice of an experienced physician.

The small treatise of Mr Lee contains a large amount of well selected information upon the uses and applications of mineral waters generally; and it is scarcely more than what Mr Lee is entitled to, when we say that he has been at great pains to make known the peculiar merits of the Continental mineral springs, and to show how their advantages may be obtained without their abuses.

The Companion to the Continent, published by the indefatigable Mr Bradshaw, is, we are given to understand, also the performance of Mr Lee. The book is one which shows great knowledge of the subject. It contains a large amount of useful information on places most commonly visited, whether for purposes of restoring or maintaining health, whether for cure, or recreation, and relaxation. The author has introduced some serviceable information on the effects of climate and travelling; and the book cannot fail to be highly useful to those who visit the Continent with either of the purposes above mentioned.

PART III.

MEDICAL INTELLIGENCE.

I. ANATOMY AND PHYSIOLOGY.

1. *Two examples of Asternia or Ektopia Cordis, illustrating the Movements of the Heart.*

In the seventy-fifth volume of this Journal, page 485, is given an instance of Ektopia Cordis from M. D. Follin, preceded by some observations on the forms of that malformation as established by Weese and Breschet. As the nature of this malformation can in no other mode be illustrated except by instances of it, it may not be unserviceable to notice here other two examples of the lesion; one published some years ago chiefly with the view of bringing forward certain reasonings on the movements of the heart, and the other quite recently.

1. *Case of Malformation with Remarks on the Circulation of the Blood.* By Thomas Robinson, M.D. of Petersburg, Virginia. (American Journal of the Medical Sciences, Volume xi. p. 346. Feb. 1833. Philadelphia.)

Early in September 1828, while confined to bed with a remitting fever, Dr Robinson received an urgent summons to visit a lady whom he had seen some days before in an advanced state of pregnancy. The messenger was also desired to say that something very extraordinary had happened. Dr Robinson accordingly repaired to the house as speedily as possible. On entering the chamber, a full-grown infant born about twenty minutes before, was presented to him. He was informed that it had struggled strongly for some minutes after its birth, and gasped frequently. The head was finely developed, the face beautiful, the trunk and extremities large and plump, except the left arm, the elbow of which was confined close below the inferior edge of the scapula, the forearm closely flexed, and the whole limb covered by the integuments of the trunk, except the hand, which appeared at the edge of the pectoral muscle. Both clavicles, the sternum and cartilages of the ribs were wanting, exposing the whole interior of the cavity of the thorax. As the shoulders and ribs were thrown back in a remarkable manner, being unconfined by clavicles, the abdomen was also open as low as the umbilicus, to which point the margins of the chasm converged from the second false rib at each side; along the right margin, the umbilical cord appeared to ramify, and there to expend itself. The investigation of this part, however interesting, was not pursued.

At the right side, close within the verge of the opening and about midway between the umbilicus and rib, a body about the size, colour, and shape of the gizzard of a large fowl presented itself; this mass attached to the margin was hard and *perfectly fibrous*, well furnished with blood-vessels, and communicated by a small branch with the funis; it had no communication with the duodenum, no trace of gall-bladder, and in no particular resembled the liver. The liver and spleen were wanting; the diaphragm cleft, a narrow strip at each side being barely cognizable. The lungs were wanting The trachea terminated about the place of the first dorsal vertebra, in a knob of cellular and parenchymatous substance not larger than a hickory nut. At the left of *the spine and higher in the thorax than usual*, was seen the heart without a pericardium. It was placed very obliquely as well as high up, so that its apex could not have struck below the third rib; it acted with surprising force and regularity between 60 and 70 times in a minute, and appeared to carry on the circulation, as the aorta pulsated very distinctly. His attention was immediately rivetted on this unexpected phenomenon—as he always wished to see things as they appear to the senses, and understand them according to the dictates of reason.

The words of the author must give the subsequent part.

I have been long puzzled on the subject of the circulation, by the discrepancies of facts and theories exhibited by systematic writers. Whenever I have found it difficult to adapt plain phenomena to preconceived notions, instead of straining the facts to fit the notions, I have discarded the notions and retained the facts. In the case before us, I shall endeavour to describe with scrupulous exactness, the action of the heart exhibited to my own sight and touch, and I think I can show that the same phenomena have been seen and felt by others, and rejected as incompatible with some favourite theory.

Viewing the heart and feeling it during the pause, although it was perhaps less hard than in either diastole or systole, yet it did not appear in that state of complete relaxation which physiologists ascribe to it; perhaps the antagonizing powers were merely in equilibrio; possibly the contracting power predominated. From this state of quiescence it would suddenly spring, dilated with surprising force, its apex elevated with a jerk, finish its double action in the twinkling of an eye, and fall back as suddenly to the stillness of death. No motion like sudden or gradual relaxation appeared in its fibres, though vigilantly watched for, and indeed expected; the diastole, which always preceded, appeared to commence in the venous sinus, and pass without interruption and with immense velocity to the apex; the systole pursued the same course with equal velocity.

When it is considered that this double action was completed in a space of time less than half a second, perhaps not more than a third, for the time of the pause appeared to be nearly, if not quite double the time of the action, you will readily comprehend the difficulty of noting particulars in their exact order; that the action passed from base to apex was visible enough—also that the diastole preceded the systole; but so rapidly did the systole succeed the diastole, that I could hardly ascertain with perfect satisfaction to myself, whether the diastole of the ventricle was *entirely* completed before the systole of the auricle commenced. My decided impression however is, that at one and the same moment the diastole is finished, and the systole commenced.

It may here be asked, if the diastole continues for so brief a moment, is there sufficient time left for the blood to occupy the cavities? I would answer, were dilatation a passive movement, it would certainly require more time for the pressure of the blood to overcome the vis inertiæ of the heart, and dilate its cavities, but when we consider that the blood is accumulating in the adjacent vessels during the pause, and under strong pressure, and when we estimate the great force and velocity with which the cavities dilate and the consequently powerful suction they exert, we can hardly doubt that they are *instantly* filled; at the moment of diastole the apex of the heart was elevated; at the moment of systole it fell back with great suddenness.

After watching the heart in situ fifteen or twenty minutes, and frequently grasping it to ascertain its force in dilating, I separated it from its connexions; its action continued without sensible diminution, either in frequency or force, and in that state it was put into the hands of different individuals, that they might be qualified to testify the fact.

I then proceeded to examine the state of the valves and septum, which were found as usual; the *foramen ovale* open, the pulmonary vein and artery, cava and aorta of the proper size. In this state, with auricles and ventricles laid open and roughly handled, the heart was thrown into a basin of cold water. After examining the other viscera for some time, I returned to the heart, and was surprised to find it still moving, feebly it is true, but with perfect regularity; the attention of the spectators was again directed to this unexpected state, and it was again placed on the hand of Mrs F., who attended the accouchement. At this period I felt myself sinking so fast, that I was reluctantly obliged to retire without inspecting the viscera of the pelvis, tracing the lesser circle of circulation, or even injecting the funis, the course of which I could not even conjecture without such aid.

In a few brief propositions I will here recapitulate the inferences which this interesting case has fully established in my mind.

1st. Systole and diastole are the natural muscular actions of the heart.

2d. The force of the diastole is equal to, if not greater than the force of the systole.

3d. There is no pause between auricle and ventricle, either in diastole or systole, the action passing from base to apex with great velocity; *perhaps* accelerated, *certainly not* retarded.

4th. There is not only no pause between the diastole and systole, but the latter almost seems to appear before the former vanishes, thus presenting the appearance of one compound action in which all the parts concerned cooperate perfectly.

5th. The diastole always precedes the systole in the natural order of action. Bichat's experiments led him to the same conclusion.

6th. Similar actions in similar parts of the different sides are always synchronous.

7th. The time of action, including systole and diastole, is less than the time of the pause.

8th. The pause succeeds the systole of the organ.

9th. The impulse of the apex against the side of the thorax is caused by the diastole of the auricles, especially the left—for as the distance of the axis of the heart from the diaphragm must be increased by the increase of its transverse diameter in diastole, and as it is firmly tied down at its base, this increase of distance of the axis can be effected only by its turning on the distended auricles—as each action of the heart is performed by a convulsive jerk, the force of the impulse is naturally accounted for.

10th. There is in the heart a *vis insita* or *vita propria*; this is demonstrated by its perseverance in action so long after its entire separation from all influence of brain, nerves, and ganglions.

The system of action deduced from this case, exhibiting a perfect cooperation of the whole organ in each action and concurrence in repose, seems much more simple and consonant to nature than the complicated series of alternations so zealously inculcated by the ancients, and which, with but very little modification, has kept possession of the schools to the present day. Passive diastole of auricles—active systole of auricles—passive diastole of ventricles—active systole of ventricles. In this series we are presented with a perpetual union of action and inertia in the same organ—one-half being always asleep, and part of the power of the waking half continually wasted in overcoming the inertia of the sleeping half. This I never could receive; but after witnessing the simple series of nature, diastole of the whole organ, systole of the whole organ, repose of the whole organ, the former arrangement appears only as a clumsy, ridiculous contrivance.

Most of the errors connected with the subject of the circulation, from HARVEY to the latest writers, may be traced to one and the same source, that unaccountable delusion, that during its diastole the heart must be in a perfect state of inertia. "Fibris enim propriis dilatari repugnat oculo." (HALLER, prim. lin.) "Neque relaxatio cordis aliquarum fibrarum actio naturalis est." (Coroll. de cord. mot.) "Diastolen nullius strati musculosi opus esse, sed relaxationem et meram inertiam." (De sang. mot. ejusq. causis.) Consequently the inert auricles must be dilated by the pressure of the blood from the contiguous veins, and consequently power lost; they must be stimulated to contraction by the blood forced into their cavities; the whole of the force with which they contract must be wasted in dilating the torpid ventricles, which in their turn must be stimulated to contraction by the fluid forced in, and by no other cause—"Non aliam causam esse hujus in motu pertinaciæ præter ipsum perpetuum sanguinis adfluxum." (De cord. mot. a stimul. nato.) Thus, from a single preconceived error, a series of absurd alternations have been ascribed to the heart. "Ut auricularum contractio cum ventriculorum relaxatione, ventriculorum systole cum auricularum diastole conjuncta sit." (De mot. sang. per cor., § xi.) A system repugnant to nature, to reason, and to the recorded observations of its advocates—a system by which the heart is deprived of about three-fourths of its power as an agent in circulation; for, by denying the power of the diastole, you deny at least half the power of the heart; and as the whole systole of the auricles is wasted on the diastole of the venricles, that, too, must be deducted, leaving only the systole of the ventricles. Such is the system of Harvey and his successors:—"Hæc in cordis motibus series Harveio quidem placuit omnibusque anatomicis ab eo qui viva animalia aperuisent." (De mot. sang. per cor., § xi.) Although so many alternations successively performed *in one second*, would seem to leave little time for pause, yet I believe all physiologists, ancient and modern, concurred in the belief of at least an apparent pause, after the systole of the ventricles.

"Expulso sanguine cor quiescit ex solo stimuli defectu" has been generally received as an axiom till the time of LAENNEC. He supposes, that the heart pauses while the ventricles are subjected to the double stimulus of distension and of the blood; this error, for error it is, grows naturally out of the theory under review. Laennec, in exploring the thorax, hears two sounds in rapid succession, followed by an interval of silence, marking, as he supposes, the pause of the heart; the first sound is accompanied by an impulse felt in every part of the arterial system; this impulse is probably the cause—it cannot proceed from the systole of the auricles whose impulse cannot reach the arterial system, it must therefore be ascribed to the systole of the ventricles; the second sound must have some active cause; diastole is passive therefore silent; the only remaining cause then, to which it can be ascribed, is the systole of the auricles. This course of reasoning seems to lead more directly than any other to Laennec's conclusion, which after all is a *non sequitur;* for I more than suspect, that the true cause has been omitted in the enumeration; besides the premises are all false. MAGENDIE admits that the heart is not entirely passive in diastole; BICHAT affirms that it is powerfully active, and the case before us confirms the fact—the diastole then is as likely to be heard as the systole.

That both systole and diastole of the ventricles are interrupted continuations of similar actions of the auricles, seems also to be established; their sounds, if any, must therefore be indistinguishably blended.

If the auricle and ventricle co-operate in the diastole, as I think I have seen them do, there is then a continued column of blood from the venous to the arterial valves; consequently, the first impulse of systole at the base of the auricle is felt through the whole arterial system. Again, considering the diastole of the auricle as the cause of the impulse of the apex against the side, (Prop. 9th,) and remembering also the celerity with which the systole follows it, (Prop. 4th,) I think the impulse of the apex against the side, and the commencement of the systole in the base, must be so nearly synchronous that no variation can be distinguished by the senses—the vibration generally perceptible in a strong pulse, may be ascribed to the increased impetuous from the superior power of the ventricle. Haller, after ascribing the stroke against the side of the systole, and especially to the curve given by that action to the apex, is obliged in candour to acknowledge that there is a further cause, to wit, the diastole of the left venous sinus—" Antrorsum *praeterea* pulsus a sinu sinistro venoso, qui eo imprimis tempore repletur." (Prim. lin. ciii.)

If this view of the circulation be correct, and I at least must believe that it is, we ought to proceed without delay to review our system of diagnosis in cardiac diseases, more especially that part which depends on the stethoscope.

That Haller and many of his predecessors, contemporaries and successors, who practised vivisections, saw the very same actions of the heart which I have described, but explained them to suit preconceived notions, might be proved by numerous quotations. They saw and felt the heart dilate with force. " Observavimus aperto pectore et pericardio, &c. &c."—" prementem digitum *valde* repellere." (Stenon. act.) " Cordum pulsat, digitum repellit non sine robore." (Haller passim. de mot cord.) Can any one believe that the finger was repelled by such a *contraction*—" ut basis apici, ventriculus ventriculo fiat proprior"—the organ literally shrinking in all its dimensions would have receded from the hand; would not forcible dilatation have been a more plausible agent of strong repulsion? but the heart is passive in dilatation, therefore the finger was repelled by contraction. They saw the heart dilate and contract from the energy of its own innate power, without mechanical distension or stimulus, " Motus cordis non turbatur capite resecto neque corde avulso." (De mot. cord.) Hundreds of similar observations on this point might be collected from various authors. They saw the same blended action of diastole and systole, as I have described, as even Harvey acknowledges, (Exerc. de motu sang.)—" In calidioribus animalibus motum cordis et auricularum videri quale simul fieri." Haller expresses himself to the same effect in the following passage:—" Dum alacriter auriculæ micabant intervallum quo earum motus præcedit cordis motum diu ægre distinxi *donec animal vegetum fuit*."* (De mot. cord.) Again—" Et *tamen candoris mei est*, phænomenon non *dissimulare* quod ipse vidi et ante me Lancisius, omnino nempe credas te videre, eodem tempore, et duas auriculas constringi et duos ventriculoso."+ (De mot. cord.) Haller in this passage acknowledges that the facts he states are inconsistent with his theory; on this ground LANCISI arraigns the series of Harvey, and proposes one perhaps nearer the truth, but still defective.

A strong argument in favour of the series of actions suggested by our case, may be founded on the structure of the organ itself. Who ever inspects with candour and attention tht structure of the valves, will find it difficult to persuade himself that they are adequate to the function generally assigned to them ; he may easily ascertain that they offer an *impediment* to the reflux of the blood, but hardly that they afford a complete obstruction ; but as the contraction of the auricles is less powerful than that of the ventricles, such reinforcement seems necessary to prevent reflux.

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* Exper. 319.

† Exper. 394.

Of this imperfection of the valves Haller was convinced by the inspection of living animals, and remarks, "Non adeo fidelem valvularum custodiam esse." (De mot. sang. ejusq. caus.) Again he says, "Neque adeo in vivo animali, tantam esse valvularum efficaciam ut vulgo creditur." If such is the inefficacy of the valves, suppose the auricle in diastole and ventricle in systole, (according to the received opinion), the reflux must seriously diminish the quantity of blood sent to the arterial system. But suppose with me, that the auricle is in a state of contraction, the only safe-guard against reflux at this time, then will the whole of the blood contained in the ventricle be transmitted by its systole to the arteries ; the auricle being less firmly guarded in this way, we perceive a reflux at every contraction, which however is checked by the column advancing in the viens ; this simple explanation of the reflux appears less objectionable than that of Magendie.

While Bichat affirms that the heart dilates with a force which no effort of the hand can prevent, is it not surprising that he has neglected to apply a power so great and so obvious to the elucidation of that obscurity of the venous circulation, on which he acknowledges authors have hitherto shed few rays of light ; to you it is unnecessary to demonstrate that the pressure of the atmosphere on the veins external to the cavities must be propagated to the termination ; that the cavities being always full, there is a continued pressure on the vessels within them ; add to this the powerful dilatation of the heart, auricles and ventricles cooperating in the action, as I have seen them, and you have all the requisites of a powerful sucking pump operating perpetually on the venous system,

That the heavy mass of blood contained in the inferior part of the trunk and lower extremities could be elevated to the heart of a tall man standing erect, by the sole force of insensible contraction of the capillaries, (as Bichat supposes), is so startling an hypothesis, that were I obliged to say *credo* to it, I should certainly add in the words of the old churchman, "*quia impossibile est.*" Besides, this insensible contraction of the capillaries has not been demonstrated ; the pump I think has been, and that its agency in the venous circulation is more powerful than that of any other cause yet assigned, or than the combined powers of all the other causes, I have no doubt ; other causes not immediately connected with our case I shall pass by with one exception.

Dr Barry has attempted to show that the venous circulation is greatly promoted by inspiration. The blood he thinks rushes through the veins towards the cavity of the thorax, to fill the vacuum occasioned by the elevation of the ribs ; he forgets that the air also rushes towards the opening vacuum, and that if two fluids, one of which is highly subtle, the other dense and viscid, present themselves under the pressure of the atmosphere at equal apertures, to an opening cavity, the quantities of these respective fluids, entering in a given time will be found in the increase ratio of their density and viscidity. If Dr B. has any doubts of this truth, as he is fond of experiments, let him procure a large syringe furnished with two pipes of equal diameter, one being curved ; let him immerse the aperture of one in fluid blood, or any other fluid equally dense and viscid, the other remaining in the air—then draw back the piston and ascertain the ratio of air to the other fluid in the machine, provided he finds any other; afterwards let him compare the lax, inelastic, long, tortuous vessels, through which the venous blood has to pass, with a wide, elastic, short, patulous pipes which admit the air. Should be afterwards demand the advance of more than half a drachm of blood for each inspiration, I shall consider him a very unreasonable person. Were it necessary at this day, it would be easy to show that the venous circulation is on the whole retarded by respiration.

The foregoing case, with the author's remarks on the inferences which he thinks it justifies, we give without desiring to express any opinion on the correctness of the latter. The facts are valuable, whatever may be thought of the inferences and reasonings of Dr Robinson.

It may be useful for readers to peruse the observations and reasonings of Dr Robinson along with those given by Dr Follin, and also with those on the following case.

2. *Observation of the movements of the Heart in a case of* Asternia, by Professor Skoda. (Comptes Rendus de l'Academie des Sciences de Vienne, 1850.)

An infant was born without breast bone. The thoracic case presented anteriorly a fissure of the shape and dimensions of the wanting *sternum*. This fissure was closed only by a thin membrane below. Some days after the birth of the infant, Professor Skoda had the opportunity of making the following observations.

At each expiration this membranous investment was depressed towards the vertebral column, and consequently the anterior extremities of the ribs were incurvated a little on inwards. On the other hand, at each inspiration this membrane was carried forwards like a bladder. In one minute the observer counted sixty inspirations and even more.

On applying the hand, the observer felt perfectly, that the heart was placed vertically; and when the finger was carried to a sufficient depth, in the direction of the vertebral column, there was felt at each *systole* the shock or impulse of the heart, above the insertion of the diaphragm, and at each *diastole*, at the level of the second or third rib. The systolic impulse was equally strong as the diastolic impulse. On placing two fingers on this space, so that the finger placed above perceived the systolic impulse, while the finger placed below recognized the diastolic impulse, Professor Skoda found that the heart during each systole glided about one inch downwards. In truth, the observer beheld upon this membranous investment moderately stretched between the fingers, the outlines of the heart, during the *systole* as during the *diastole*, and from this it may be concluded that the impulse of the heart against the two points indicated was not the result of an enlargement or of a diminution of this organ, but proceeded solely from a displacement of the heart.

If the observer did not touch the investing membrane, he remarked, during expiration at the moment of the systole, the outlines of the heart shown by an elevation from above downwards upon the membrane pushed forward; during the *diastole*, on the other hand, there was seen a depression from below upwards. During inspiration, the outlines of the heart were in like manner indicated at two times upon the membranous investment when carried towards the vertebral column.

The heart beat from one hundred and twenty to one hundred and thirty times each minute, and was moved from above downwards with considerable exactness upon the middle of the fissure, when the infant was lying upon the back. If on the other hand, the infant lay inclined upon one side, then in its movements, the heart deviated slightly from the median line, and that toward the side on which the infant was placed.

This case is regarded by the author as fully confirming the explanation which he has given of the movements of the heart.

The reporter in the *Archives* remarks on this case, that it serves to show the facility with which some observers see proofs in favour of a previously received opinion. As the sternal opening, he remarks, was not complete, but was shut up by means of a membrane, it was impossible that the heart could be watched with sufficient precision in its movements. Neither could the heart be seen, nor held by the hand, so as to appreciate in an exact manner its times of contraction or of dilatation. Even though this impediment did not act in other cases of EKTOPIA, such as that of M. Follin, yet in most of these cases, the observations are made with so great difficulty and so little exactness, that they are necessarily imperfect and unsatisfactory, and cannot justly be applied to sustain any given theory. The observation is certainly well founded, that often the opinion of the observer is substituted for observed facts.

He allows, nevertheless, that this objection does not apply to the case of Dr Robinson, in which the heart, being completely exposed, it was possible to follow its movements in all directions.

It is further added, that in two cases of Ektopia as well marked as that given by Dr Robinson recently presented to the academy, and of which that of M. Follin is manifestly one, it was possible to demonstrate the fact, that the heart went from its state of repose by a sudden enlargement of all the diameter of the ventricular portion, with elevation of the apex, and that at this instant, these movements constituted not the *systole* as is believed, but the *diastole*. The systole terminated rapidly the *diastole*, and its character was the retraction of the apex, and the return of the dilated ventricle to the normal state. This order in the movements of the heart was observed by several physicians present, who witnessed the fact.

It will be important, therefore, if opportunity of observing similar cases shall present itself, to be known what it is requisite to ascertain, to determine the nature of the ventricular movement which follows the state of repose. Is it constituted by a sudden enlargement of all the diameters, exactly like the arterial pulse, and the movement which is usually regarded as the *systole* is not, on the contrary, the *diastole*, as Dr Robinson affirms, and as appears to have been seen in the cases presented to the Academy of Medicine ?

Note.—It may be proper to mention that most of these passages in Latin quoted by Dr Robinson are from one or other of three Essays of Haller, *de Motu Sanguinis per Cor.* the Experimental Essay *de Cordis Motu a Stimulo Nato*, and the Experimental Essay *de Cordis Motu.* OPERA MINORA, Tomo ii. Lausannae, 1763. 4to.

These passages are so erroneously printed in the work containing the Essay of Dr Robinson that they are altogether unintelligible.

2. *On the* PAROVARIUM *in the Female ; the organ analogous to the* EPIDIDYMIS *in the Male.* By Professor KOBELT of Fribourg. (Der Heben-Eierstock des Weibes, das Langstvermisste Seihenstuck des Heben-Hoden des Mannes. Heidelberg, in 8vo, 52 seite, Fig. 1847).

Notwithstanding the diversity of opinion entertained by embryological writers on the anatomical and physiological signification of the *Corpora Wolffiana* to the genito-urinary apparatus, they are nevertheless generally agreed, in considering these bodies as organs peculiar to the life of the fœtus, and disappearing in both sexes without leaving any perceptible traces behind them. Hypotheses rather than facts have led physiologists to admit the *vasa aberrantia* of Haller of the epididymis in man and the *organ* of Rosenmüller, as well as the ducts of Gaertner in the female, to be the presumed remains of the Wolffian bodies. Meanwhile it is possible to demonstrate anatomically the fact, that these products, which have been hitherto considered as temporary in both sexes, are not only continued during the whole duration of life, but further that they arrive at the greatest sexual maturity, and that they begin only to undergo changes, and wither, as it were, after the cessation of the genital functions, without, however, disappearing completely. The author gives in the following sentences the facts which he believes he has proved in the course of his Treatise.

1. In the earliest periods of intra-uterine life all individuals present a true sexual indifference, which hitherto has been admitted rather hypothetically than strictly proved.

2. This sexual indifference, or absence of sexual distinction, is constituted by the temporary co-existence in each embryo of all the elements constituting the generating organs of both sexes.

3. The generating gland, the point from which proceeds the whole of the sexual difference, may become either a Testicle or an Ovary.

4. By means of the excreting ducts of this generating gland, that is to say, by means of the canal of Muller in the female, and of the excreting duct of the *Corpus Wolffianum* in the male, and, on the other hand, by means of the mammary rudiments, which exist in all subjects, each embryo, in proportion as it becomes developed, may assume the attributes which characterise the one sex or the other.

5. The sexual difference is constituted and is pronounced by the increase of one of these conduits, and by the arrest of developement in the other.

6. In the male, the *Corpus Wolffianum* never entirely disappears in all its parts; but it constitutes in great part the epididymis, and this it does in the following manner.

7. Its *cul-de-sacs*, or blind tubes (*caeca*), constitute the eighteen or twenty of the *Coni vasculosi* of the epididymis. Subsequently these communicate freely, like *Vasa efferentia*, with the *Rete vasculosum Testis*.

8. The superior *cul-de-sac*, and the ampulla of the excretory duct of the *Corpus Wolffianum* are obliterated, or become, in consequence of their transformation hydatiform vesicles, situate upon or in the epididymis.

9. The inferior *cul de sac* of the *Corpus Wolffianum* disappear or are converted into the *Vasa aberrantia* of Haller, hitherto unknown in their origin and signification.

10. The excretory duct of the *Corpus Wolffianum* constitutes the canal of the epididymis and the *Vas deferens*.

11. The *ampulla* of the ducts of Muller becomes the hydatid of Morgagni; this conduit is at a later period itself found in the prominence of the anterior margin of the epididymis.

12. In the female, the *Corpus Wolffianum* never disappears in all its elements; but it concurs in the formation of the new body discovered by Professor Kobelt, and by him named PAROVARIUM. This body is situate between the Fallopian Tube and the Ovary, in the ligamentous folds designated by the name of *Alae Vespertilionis* or Bat's Wings,

13. The middle *cul-de-sac* are transformed into the eighteen or twenty *Coni Vasculosi*, and penetrate by converging into the *Hilus Ovarii*. These are the analogues of the *Coni Vasculosi* in man.

14. The superior *culs-de-sac* also disappear, or at a later period constitute the hydatid vesicles so frequently observed upon the fringes of the Fallopian Tube.

15. The inferior *culs-de-sac* disappear. They correspond to the *Vasa Aberrantia* of Haller.

16. The excretory duct of the *Corpus Wolffianum* undergoes an arrest of developement. The inferior extremity disappears.

17. The conduit or duct of Muller becomes the Fallopian Tube. Its terminal *ampulla* constitutes the hydatid of the tube, the presence and origin of which were hitherto unexplained.

18. The *Parovarium* is subjected to the same conditions of growth and arrest of developement as the Ovary itself.

19. The hydatids in the *Alae Vespertilionis* are merely the enlarged rudiments of the *culs-de-sac* of the *Parovarium*.

Professor Kobelt has examined the remains of the *Corpus Wolffianum* in a great number of Embryos of Mammiferous animals; and found the following state of phenomena. In the guinea pig they were as a bundle of secreting glandular *Canaliculi* placed at the entrance of the Ovary to which they were adherent. In the hare he recognised the elementary disposition of the two ducts of the generating gland, as well as their ulterior transformation. In the pig there are seen traces of the ducts of Gaertner and of the radiating appendages hitherto unknown. In the RUMINANT order, upon the *hilus ovarii*, there is a small sac, which, like its tubiform appendages, are nothing but the remains of the superior blind sacs and their ducts of Gaertner, the persistent excretory ducts of the Wolffian body; the radiating appendages of this part are the remains of their numerous inferior blind or shut sacs.

20. The duct of Gaertner is nothing but the *vas deferens*.

22. The occurrence and phenomena of *Hermaphrodism* may therefore be explained by the presence, in all embryos, of all the constituent elements of the generating system, male and female.

21. The frequent occurrence of hermaphrodism in the apparent coexistence of male and female organs in Ruminant animals, the complete state in which it presents itself in this order of animals, may be understood by the persistence in them of the two excretory ducts, male and female.

These preliminary communications, says Professor Kobelt, in concluding his Memoir, shall be completed by a full Memoir on Hermaphrodism. The researches, the substance of which has been given in this summary, have received the approbation of the German physiologists, and are admitted as integral parts of physiological science in the classical treatises of the Ultra-Rhenish countries.

3. In the *Archives Generales* it is recommended to compare the researches of Professor Kobelt with the results of other inquiries recently made known in an able Inaugural Dissertation by Dr E. Follin.

The object of this anatomist was to study the developement of the genito-urinary apparatus, to show in what manner the sex of the fœtus is determined, and to explain by these embryological researches the assemblage of anatomical facts, improperly denominated hermaphrodism. Only one part of these results, namely, those relating to the *Corpora Wolffiana*, has the author hitherto published. Of these the following summary is given by the author.

1. The *Corpora Wolffiana* are organs which arise by *two distinct parts* on each side of the vertebral column, and extend throughout the whole length of the visceral cavity of the embryo.

2. These bodies are not a production from any of the folds of the *Vesicula Umbilicalis*, as Baer, Burdach, Rathke, Valentin believed; nor from the Allantois, as was thought by Reichert.

3. They are formed by a special, express deposit of organic cellules.

4. They appear before any of the elements of the internal genital organs and the kidneys.

5. The genital gland (Testicle or Ovary), is developed independently of these bodies, and at their internal margin.

6. The kidneys are formed by several distinct lateral tubercles at the posterior surface of the *Corpora Wolffiana*, which do not proceed from them, as Wolff formerly and Arnold recently had believed.

7. These *Corpora Wolffiana* are formed by tubes at first straight, then more and more flexuous, terminating in closed or blind sacs, placed nearly transversely, and not anastomosing with each other.

8. Vessels are distributed between them, but in the MAMMIFERA do not serve to form glomerules, as Rathke has represented.

9. All these tubes terminate in an excretory canal, situate at the external margin of the *Corpora Wolffiana* in the MAMMIFERA, and penetrating by the inferior in BIRDS.

10. At a certain period of the process of development the *Corpus Wolffianum* is so large that it conceals its own excretory canal; hence proceed numerous mistakes upon the transformation of the duct of Wolff into Fallopian tubes on the one hand, and *vas deferens* on the other.

11. The excretory duct of the genital gland (Testicle or Ovary), is developed independently of the ducts of the *Corpora Wolffiana.*

12. Primarily, a complete independence subsists between the male or female genital organ and its excretory duct.

13. In the male sex this canal approaches to the genital gland (testicle), unites itself to it by a sudden hook, then is turned into indefinite folds, and forms the epididymis.

14. In the female sex this duct is widened slightly at its superior extremity, and remains separate from the genital organ.

(It is scarcely necessary to observe that this proposition applies only to the general and habitual state of the parts, excepting during the act of sexual union and conception, at which it is known that the upper end of the tube is temporarily united with the generative gland, and clasps it firmly.)

15. All these phenomena take place beyond and without the *Corpora Wolffiana.*

16. This primary disposition of the generative gland and of its tube furnishes the key to explain all the sexual anomalies improperly denominated *hermaphrodism*

17. The *Corpora Wolffiana* appear in FISHES to be persistent in the state of kidneys.

18. The *Corpora Wolffiana* exist in REPTILES. [Batrachoid and *Ophidial,*] and in their existence is observed to be prolonged under the shape of two whitish masses, situate beneath the branchial cavity.

19. Very voluminous in BIRDS, traces of them are still observed during the first years of life.

20. The *Corpora Wolffiana* are not completely obliterated at birth in the human race.

21. In the female, they are continued in part under the form of small *canaliculi*, situate in the substance of the broad ligament in the vicinity of the Ovary.

22. These *canaliculi*, by their disposition, their forms, their structure, and their relations are independent of the neighbouring organs. M. Follin has, by a series of preparations, shown, that these were only the last vestiges of the *Corpus Wolffianum.*

23. These *canaliculi* are distended by liquid, and often form those cysts which are seen in this part of the broad ligament.

24. The superior extremity of the duct of the *Corpus Wolffianum* is often marked by a pediculated or stalked vesicle, which is inserted upon the fringe of the Fallopian tube.

25. In men we find remains of the *Corpus Wolffianum* at the level of

the head of the epididymis. These vestiges are formed by some diverticular *canaliculi*, by the *vas aberrans* of Haller, and by the hydatid of Morgagni, which to M. Follin appears to be the analogue of the pedunculated vesicle, which is also constantly observed in the female.

26. The *canaliculi* of the Wolffian Body do not form, as Kobelt represents, the *vasa afferentia* of the testicle.

27. These diverticular *canaliculi* in the head of the epididymis are sometimes swelled by a deposit of fluid, and they form those cysts so frequent at the head of the epididymis.

28. The Wolffian Body leaves also in certain Mammiferous Genera, traces of its existence.

29. In the pig and in the cow, we see them in the neighbourhood of the free end of the Fallopian tube, under the form of some *canaliculi*, similar to those in the human female. These *canaliculi* are often masked by deposits of fat.

30. The excretory duct of the Wolffian Body is continued in the state of a long hollow cord, which at length opens at the lower extremity of the vagina and bears the name of the Duct of Gaertner.

31. In the Guinea Pig, as in the Rabits, are found also remains of the Wolffian Body after birth; there are small *canaliculi* situate at the same place as in the human female. As to the excretory canal, it is absorbed. In none of these animals does the Wolffian Body serve to form the kidneys or the genital organs.*

4. *On the Transformation of Muscular Tissue into Fat.* By Dr BARDELEBEN *of Greifswald*, (Jenaische Annalen, 1850, and Archives Generales, Mars 1851, p. 331.)

In the seventy-first volume of this Journal Mr Hallett published in 1849 an excellent Memoir on the conversion of the Muscular Tissue into Fat; and in the former part of the present volume was given the most essential part of the researches of Dr Quain upon the adipose transformation of the heart.

In all the instances of transformation of the muscular tissue into fat hitherto observed, defect of exercise of the muscle has been remarked as an occasional cause, and considered as a sufficient condition. In the instances observed by Dr Bardeleben, he found, as a second occasional condition, a derangement in the function of respiration, owing to compression exerted upon the lungs by the thyroid gland, amounting to the weight of two hundred and fifteen (215) grammes.

The microscopical examination of the degenerated muscles showed the following changes. Nowhere were there traces of primitive muscular *fasciculi*; there existed only fat and fibrous tissue in small quantity. The fat presented itself partly under the shape of minute drops, and principally as fat cells, and this in the interior of the sarcolemma of the primitive *fasciculi* which previously existed. The fat-cells or droplets of fat were disposed in ranges accurately bounded of the breadth of the primitive muscular *fasciculi*, closely packed against each other. Each of these ranges was limited on each side by a straight ray or line with well defined outlines. This ray was still more easily and distinctly recognized in the points, where, in consequence of compression, the contained fatty matter had been swept away or crushed. These heaps of adipose tissue were then contained within amorphous tubes, destitute of structure, which the author considers as the investments or sheaths of the primitive muscular fasciculi deprived of normal content.

If we adopt this mode of explanation, then it follows that the fatty tissue must have taken the place of the muscular tissue. This mode of considering the fatty degeneration of muscular tissue appears so much the more admissible, that in recent times, chemical and microscopical researches have furnished proofs of the possibility of the transformation of a compound of proteine into fat.

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* Recherches sur les Corps de Wolf. These par M. Le D. E. Follin. Paris, 1850. 4to, pp. 70.

The colour of the degenerated muscles was of a straw-yellow; their surface became hard in the air and transparent.

5. *On the part performed by the Chyliferous Apparatus in the Absorption of Alimentary Substances.* By M. Cl. Bernard. (Report of Proceedings of Academy of Sciences at Paris for 9th December 1850. Archives Generales, 4ieme Serie, T. xxv. January 1851.)

In this Memoir M. Bernard proposes to fix by direct experiments the nature of the nutritious principles which are absorbed and carried exclusively by the chyliferous vessels, with the view of defining in a precise manner the signification of the word *chyle*, and determining whether there really are alimentary substances which absolutely escape the operation of venous absorption, and consequently avoid passing through the liver before arriving at the lungs.

The alimentary articles subjected to the acts of digestion are finally referred to three principal substances; saccharine matter, albuminous matter, and fat, reduced to the state of emulsion. Upon each of these principles the experiments of M. Bernard have been directed.

1. Upon introducing into the stomachs of different mammiferous animals cane sugar, M. Bernard always found this principle in the blood of the *Vena Portae*, and in no instance in the chyle of the thoracic duct. Sugar then is absorbed exclusively by the Portal Vein, and of necessity passes through the liver. This is one condition of the assimilation of this substance. Decisive experiments have proved to M. Bernard that this transition of sugar through the liver has the effect of causing this article to undergo a very important modification. If we inject into the venous blood of a dog, by a vein of the surface of the body, a solution of two or three grammes of cane-sugar, this substance, instead of being assimilated, is discharged at the end of some instants, by the urinary secretion. If, on the contrary, this solution be injected by a branch of the Portal Vein, in such manner that the saccharine material passes forcibly into the liver before getting into the general venous system, we find that the sugar is not eliminated, that it remains and is assimilated in the blood, exactly as this takes place, when the substance is absorbed by the aid of the normal process of digestion. Sugar which is absorbed by the chyliferous (lacteals) vessels, would escape assimilation.

2. The same is the case with albumen of egg injected into the general venous system, and into the *Vena Portae*. In the first case, the urine becomes albuminous; in the second case, the albumen remains in the blood, modified, as it has been by the action of the liver. These facts tend to prove, according to the opinion of M. Bernard, that the albumen of egg is probably not identical with the albumen of the blood, and that it is absorbed exclusively by the Portal Vein.

3. In a previous Memoir M. Bernard has shown that neutral fat substances, in order to be capable of penetrating into the chyliferous vessels, must have previously undergone the emulsive action of the Pancreatic juice. These vessels assume a whitish milky aspect; and it is possible to follow the course and transition of the fatty matter from the intestine to the left subclavian vein, into which it is poured by the thoracic duct. Fatty matters, therefore, have no need to traverse the liver; and, in truth, M. Bernard having injected into the jugular vein, in large quantity, various fatty substances converted into emulsion by the pancreatic fluid

of a dog, never saw after these injections the urine to contain fat and become chylous. Nevertheless, contrary to what is the case with saccharine and albuminous matters, which are absorbed exclusively by the Portal Vein, fat is absorbed at the same time by the Portal Vein, and by the system of the chyliferous vessels. This is proved by microscopic inspection and by experiments. As to other points, if in the Mammiferous tribes we may ascribe to the chyliferous system an evident part in the absorption of fat, the same is not the case in a great many of the Class of Birds, in which it is impossible to establish the existence of any trace of chyliferous apparatus. M. Bernard has caused pigeons, common fowls, and other birds to swallow fat, and upon putting these animals to death in the midst of the process of digestion, he has never been able to find the slightest appearance of chyle in their intestinal lymphatic vessels, while the blood of the Portal Vein contained a great amount of fatty matter in the form of emulsion.

"In final conclusion," says M. Bernard, "there is therefore only one alimentary substance, namely fat, in the absorption of which we can prove in an evident and substantial manner the action of the chyliferous system; and notwithstanding, this function, which in the Mammiferous tribes is shared with the Portal Vein, is completely annihilated in a great number of animals, which nevertheless digest and absorb in a complete manner fatty articles of food. What signification, then, will it be necessary to attach to the word *Chyle?* I have shown in my Memoir upon the Functions of the Pancreatic Juice,* after M. Magendie and other physiologists, that fat bodies alone give to the chyle the specific characters of a lactescent or milky fluid. When an animal takes for food nothing but fatty matters, the content of the chyliferous system ought apparently to be in the main, only lymph augmented by the addition more or less considerable of a quantity of fat. In instances of complex feeding, the question arises, is the chyle anything else? The experiments which I have related in this Memoir would tend to make us think that it is not. Nevertheless, in order to resolve a question like this, and to change in a manner so perfect the acceptation which has been so long assigned to the word chyle, it will be further necessary to add new comparative observations upon the chyle and the lymph, and upon the chyliferous apparatus considered as a department of the general lymphatic system."

To the foregoing observations it is unnecessary to add much. Two points only require notice.

It may be proper to observe, that the distinction or arrangement of alimentary substances into three orders, saccharine, albuminous, and fatty, was originally made in 1827 by Dr Prout in his Memoir in the Philosophical Transactions. This it would have been superfluous to notice were it not, that the distinction is introduced above, as if it were entirely new.

It seems scarcely possible to expect that anything else could happen from the injection of pure cane sugar than elimination. All experiments show that pure sugar is either not easily assimilated by the animal tissues or is actually hurtful. In the stomach it is often converted into acetic acid, lactic acid, to the disturbance of digestion. It seems very doubtful whether pure cane sugar in its isolated form can nourish and sustain life for any length of time. It is certain that never in nature is it presented in this form as food, but always either in the form of starch, the flour of the cereal grains, or the usual culinary vegetables, as potatoes, turnips, carrots, parsnips, and similar articles. Upon such articles it is known that animals subsist and maintain health and strength. But on the other hand the experiments of Dr William Stark formerly, and subsequently those by Magendie and several others, showed clearly that pure sugar does not support animal health and strength, but if continued injures the one and impairs the other.

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* Edinburgh Medical and Surgical Journal, Volume lxxi., p. 182. July 1849.

6. *The Inutility of the Bile in Digestion, properly so called*, by M. BLONDLOT. (Proceedings of the Academy of Sciences, 23d June. Archves Generales de Medecine. July 1851. P. 370.)

In a memoir on this subject, read to the Academy on the 23d June 1851, M. Blondlot, known as the author of a Treatise on the Physiology of Digestion, published in 1843, has brought forward several new and rather singular views.

In a memoir published in 1846, with the title of *Essay upon the Functions of the Liver*, M. Blondlot proposed for resolution the question—Is the bile a product entirely excrementitial, or has this fluid an essential part to perform in the function of digestion? In this performance the author arrived at the conclusion that the bile is truly a sort of *detritus* or refuse article of the animal economy, which thus gets rid of it by means of the intestines, and that it exerts no chemical action of any importance upon the alimentary articles with which it comes in contact.

In the present memoir, M. Blondlot calls anew the attention of physiologists to the leading fact in his first essay, which is the establishment in living animals of permanent *fistulæ* conveying outwards the entire amount of the bile, the normal duct of which had been previously obliterated. After a great number of unsuccessful attempts, M. Blondlot has succeeded in establishing *fistulæ* of this sort upon two dogs. One of these animals having been put to death, about one month after the operation, it was possible to observe that digestion was very well accomplished, though the occlusion of the *ductus choledochus* was perfect. The other dog was allowed to live, in order to trace as far as possible the results of the experiment.

She was a spaniel bitch, which might be about three or four years old at the time of operation. She was carried to the country and lived there in freedom. Her health was so good that she hunted with ardour part of the time, and every year she gave birth to whelps. The appetite was excellent and the alvine discharges void of colour. Meanwhile the bile did not cease to flow by the fistula with its habitual characters, but in a manner in some sort intermittent; that is to say, that when the animal was fasting scarcely did a few drops flow; while some minutes after being fed, the fluid issued in abundance and continued so to do during the whole course of digestion. This state of matters lasted for five years; after which the animal, which seemed for some time to pine away, terminated by dying, without presenting any remarkable incident.

Anatomical examination showed the organs of the chest and abdomen to be sound, with the exception of the liver, which was shrivelled, hard, sprinkled on its surface with yellow points, and presented the aspect of livers affected with *Kirrhosis*. Firm adhesions united the fundus of the gall-bladder to the abdominal parietes at the situation of the *fistula*. This receptacle, though empty, had retained its pyriform cavity and its dimensions, which depended on this circumstance that its walls were considerably thickened. The cystic duct was very much dilated, and seemed to form the continuation of the hepatic duct. At their point of union was very distinctly seen the origin of the common bile duct, which ended abruptly in a shut sac. These different canals were very much dilated, and their walls were thickened. On the side of the duodenum there was no longer perceptible the smallest vestige of the common bile-duct (*ductus choledochus communis*); and the most minute dissection was unable to discover any thing which resembled a supplementary duct.

From this experiment the author deduces the conclusion, that the bile performs no essential part in digestion; that it ought to be considered as a refuse article, which, before being entirely expelled from the economy, performs still some services of very secondary importance, either by contributing with the other mucous fluids to convert fatty matters into an emulsion, or by protecting the intestines against the acrimony of chyme, the acid of which it partly neutralizes, and the progress of which it promotes by its unctuous nature. This he thinks is sufficient to explain the constant situation of the excretory bile duct immediately below the stomach.

At a subsequent meeting of the Academy, on the 8th July, M. Semanas of Lyons communicated, *upon the Functions of the Liver during Digestion, and upon the uses of the Bile for Digestive Albumen*, a Memoir in which he opposed and questioned the correctness of the conclusions deduced by M. Blondlot.

M. Semanas does not dispute the correctness of the experimental fact itself; but he maintains that M. Blondlot had deduced illegitimate conclusions.

M. Semanas reproduces in this Memoir his ideas formerly published upon the subject of digestion, and gives a summary of these ideas in the following propositions.

1. Digestion properly so called, that is to say, abstracting from the processes relative to the conbination of the alimentary articles, consists of two principal periods; *a*. intestinal or nutritive digestion; and *b*. hepatic or secretory digestion.

2. Hepatic or secretory digestion, the only one which the author considers in this Memoir, comprehends the preparation and absorption of albuminous materials.

3. The preparation of albuminous materials previously conveyed by the Portal Vein is performed in the interior of the liver by the bile which these materials find in this organ, which is united with them and alkalinizes them with a view to their digestive absorption.

4. Digestive absorption of albuminous materials is performed in the interior of the liver by the agency of the hepatic lymphatic vessels.

5. From this it follows, that the liver may be the digestive organ of albuminous articles of food, and the bile, the principal action of which is consequently within the liver and not outside that organ, is the dissolving alkalinizer of digestive albumen.

6. Lastly, the hepatic and cystic ducts are the evacuants of excrementitial bile, and very probably also the agents in the preparation of albumen, and of bile not excrementitial.

The reasonings and deductions of M. Blondlot have been here given as they are reported, without expressing any opinion as to their conclusiveness, and leaving that to be done by the opposing inferences of M. Semanas. It can scarcely, however, escape the notice of the most superficial reader, that M. Blondlot has not only, as M. Semanas represents, deduced from his single experiment conclusions not legitimate, but that he has strangely enough overlooked several important circumstances, and in his eagerness to prove that the bile is useless, he has reasoned in a manner which tends to make all reasoning on physiology of doubtful service.

It may be observed, in the first place, that though experiments on the lower animals are within certain limits important and necessary to determine certain points, yet it is impossible to apply the results obtained by these experiments without any qualification or condition to what takes place in the human body. The bodies of animals suffer great injuries and mutilations, without inducing in them those serious and often fatal effects which follow in the human body. To maintain, that because an open fistula from the liver and gall-bladder carrying the whole bile without the body and entirely away from its usual route into the duodenum, does little or no injury to a dog, this fluid is of no use, and may be entirely dispensed with in a man, is a proposition so extravagant, that it can scarcely be received for one moment by the mind.

Without neglecting experiments, are we not to draw conclusions as to the utility or inutility of certain organs and their secretions from the phenomena daily afforded by the effects of disease. Does jaundice produce no bad effect upon the health of the sufferer? Is the man in whom the bile does not get into the duodenum in the same healthy state as the man in whom that fluid does reach the intestinal tube. Is *kirrhosis* a disease of no importance, and does it produce upon the digestion and the general health no unfavourable impression? Do people never die with jaundice or kirrhosis, that M. Blondlot thinks it a matter of indifference whether bile be secreted or not, or whether, when it is secreted, it is altogether of no moment whether it is mixed with the food or not. Until these questions be answered, without qualification or condition, in the affirmative, we think it will be safe for physicians to believe that the bile does perform some useful service, though it may not be possible to specify all the services which it performs.

Both in jaundice, in which the bile is secreted, but prevented from entering the duodenum, and in kirrhosis, in which it is probably not secreted in its proper form of bile, there is so much debility both of the alimentary function and of the general health, that it seems impossible to deny that the proper secretion of the bile is of the utmost importance both to digestion and nutrition, and consequently to the general health. The secretion of the bile appears to be serviceable in two modes; first, in removing from the blood principles which, if retained, must prove to be injurious; and, secondly, in effecting in the chyme, as found in the duodenum, certain changes, which, if not effected, render the chyme less nutritious, or at least less fit for the purposes of nutrition than it is in the normal state.

If it be objected that in *kirrhosis* the mischief appears to be done by the obstructed circulation in the liver, and by the dropsical accumulation in the abdomen; the answer must be, that dropsical accumulations from other causes in the abdomen do not kill with the same certainty or the same rapidity as that arising from *kirrhosis*, and that the injury inflicted on the digestive function and on the general health is invariably in the proportion in which the structure of the liver is changed, and consequently its function is suspended and interrupted. Scarcely any disease can be named, which so certainly disorders and impairs digestion and nutrition, and so certainly carries the patient to the tombas *kirrhosis* in a perfect form and complete degree.

Further, if we take the story of the dog on which M. Blondlot performed his experiment, as it is recorded, it cannot be said that this experiment was without most hurtful effects. Surely neither the decaying health and pining of the animal during life, nor death itself can be termed trifling results; and to tell us that the animal died, without presenting any remarkable incident, after she had been wasting away for weeks at least, if not months, is a statement which, if M. Blondlot expects the profession to receive, he must possess a mind of most singular and unusual conformation. Even when the uneducated observe either an animal or a human being slowly but surely pining away, the natural conclusion is, that there must be at work some very severe and unmanageable disease upon some of the organs essential to health. But when death is observed to close the whole scene, none but one without a mind possessing the usual amount of reasoning power, could infer, that all this mischief, whatever it might be, was, unimportant, not essential, to health and life, and, for any thing that we know to the contrary, might have allowed the animal or the man to go on living as he did for several years. The natural, and it appears to us the necessary, inference is, that, as effects must be proportionate to causes, the main cause of the previous pining and wasting, and the final death was a serious though slowly advancing lesion of some organ essential to life.

Even on this point we are not left by the experiment of M. Blondlot and the dissection altogether in doubt and in obscurity. Though the organs of the chest and the abdomen were sound, the liver, it is said, was shrivelled (*ratatinée*) on the surface, hard, sprinkled with yellow points, and presented the appearance of livers affected by *kirrhosis*. If any thing is pointed this is. All the organs are sound; the liver alone presents a most serious change. Does not this show at once, without ambiguity, that, so far as human reasoning can go, it must have been the change in the structure of the liver which caused the animal to pine and waste away, and finally to die. So far as we perceive, no other conclusion can be deduced.

Did any person, the question may be proposed, ever live beyond a few months in whom the liver is in a state of considerable and general *kirrhosis*? From what we have witnessed of this disease, we can have no hesitation in answering in the negative.

Upon one point in the mechanism of the biliary apparatus M. Blondlot has not even touched. We know not very positively and accurately what are the uses of the bile *within* the body. Upon these we are permitted to do little more than to reason, to speculate, and to conjecture. But this we know, that if the bile be not excreted and in some form eliminated, its presence within the economy gives rise to most pernicious effects. This is proved both by the phenomena of long continued jaundice, various diseases of the liver, in which the secretion is retained, by those of *Kirrhosis*, and by those of atrophy of part or the whole of the liver. One reason why the spaniel of M. Blondlot lived so long, was the circumstance, that the bile which was formed, was not retained, but was conveyed out of the system by means of the *Fistula*. But, again, when the liver was so much altered in structure, that it no longer secreted proper bile, or was able to secrete it for the purpose of excretion, the animal began to waste away, and finally died. If there be any analogy between the kidneys and the liver, it may be inferred, that while the liver was getting into the hardened, shrivelled state, it was unable to take from the blood the materials of the bile, and therefore these were retained within the system, that is, elements which require to be eliminated were not eliminated, and consequently acted as irritant poisons, slowly indeed, but not less certainly.

If any doubt be entertained as to the validity of these inferences, it must be removed by referring to the phenomena, as already said, both of Kirrhosis and of Atrophy. Of the latter lesion an instance is recorded in the seventy-third volume of this Journal, which shows its harmful effects upon digestion and nutrition in general. Though incurable ascites was one prominent effect of this lesion, there was besides great debility and marasmus, which no doubt contributed to the fatal termination.*

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* Account of an Instance of the Occlusion and Obstruction of the Cavity of the Vena Portæ; with remarks on its probable causes and effects. By David Craigie, M.D. &c. Edinburgh Medical and Surgical Journal, Volume seventy-third. April 1850. P. [illegible] and [illegible].

In other respects the most serviceable and instructive lesson taught by the experiment of M. Blondlot is the curious fact of the producton of the state like *kirrhosis* after the establishment of the *fistula.* The question may be asked, was this condition of the liver,—shrivelling and induration,—caused by the diversion of the bile from its usual channel, or was it the effect of the inflammation extending from the biliary ducts by the capsule of Glisson to the liver itself. One or two experiments of this kind might lead to instructive results as to the origin and causes of *kirrhosis.*

From the fact of the animal, living five years with this hepatic *fistula*, in the apparent enjoyment of health and some degree of strength, no other conclusion can be deduced, than that it shows what powerful efforts the system makes in animal bodies, in order to counteract and mitigate the effects of great injuries and lesions.

7. *On the Pancreatic Secretion.* By M. Colin, Chief of the Department of Anatomy at the Veterinary School of Alport. [Report of Proceedings of Academy of Sciences, Archives Generales de Medecine, 4ieme. Serie, Tome xxv. p. 487. Abric Paris, 1847.]

M. Colin on the 10th of March addressed to the Academy *Some Experiments on the Pancreatic Secretion in the large Domestic Ruminants.*

The experiments which led M. Bernard to the discovery of one of the most remarkable properties of the pancreatic juice, possessed sufficient interest to induce M. Colin to attempt to perform similar experiments upon animals, which by their regimen and mode of digestion, differ as widely as possible from the Carnivorous tribes, in order to ascertain, whether in all indiscriminately this fluid is endowed with identical characters and properties.

It appeared difficult *a priori* to admit that in the herbivorous animals, whose food contains only a small proportion of fatty matters, the fluid secreted by the pancreas ought nevertheless to possess emulsive properties, similar to those which it possesses in the Carnivorous tribes, upon which M. Bernard performed his experiments. This identity in action, nevertheless, is incontestible; and its positive demonstration would alone be sufficient to render new researches legitimate. But it still was an object of enquiry, to throw some light upon the phenomena so obscure, and so little accessible of this secretion, to attempt the quantitative determination of the product secreted in a given time, to discover whether the secretion is continuous or intermittent, whether it is more abundant at any one given period of digestion, than at another, and whether the flow of its product externally causes more or less considerable disorder in the process of intestinal digestion. With the purpose of collecting the elements which might lead to the solution of these several questions, M. Colin undertook the experiments, which he described to the academy. The principal results may be digested in the following propositions.

1. The quantity of pancreatic liquid secreted in a cow of medium size is very considerable, in so far as it amounts in the course of one hour to two hundred and seventy-three grammes (273). This number presents nothing astonishing, because twelve thousand and five hundred grammes of fodder, (12,500) form the daily allowance of one animal of the bovine family; and according to the analyses of M. Boussingault, this quantity of vegetable matter contains five hundred (500) grammes of fatty matter, which in order to be converted into an emulsion, require to be subjected to the operation of fifteen hundred (1500) grammes of pancreatic fluid.

2. The secretion, instead of being continuous and regular, undergoes variations which give to it an intermittent type. If at one given moment it is very abundant, we see it very soon diminish, become very inconsiderable, or completely cease, yet resuming afterwards an increas-

ing progression, which, after attaining its maximum, again undergoes a diminution.

3. The most elevated degree of the secretion coincides most frequently with the end of the process of rumination, and the movements which follow it. It also corresponds sometimes to the hours during which the animal is eating.

4. The fluid secreted presents its perfect emulsive properties only in the first times. In these it is thick, viscid, contains a large proportion of albuminoid principle, and it forms by agitating one part of olive oil with three of juice, a complete emulsion, which remains constantly homogeneous.

5. The fluid which is obtained only one hour and a half after the establishment of the pancreatic fistula is already at this time less albuminous, and is unable to produce a homogeneous emulsion, even when its proportion in the mixture is double or three times that which it previously was. In other respects these properties are weakened in proportion as the fluid becomes more watery; but it never entirely loses them at any period of the course of experiment.

6. In consequence of its contact with oil, the pancreatic fluid, which is always alkaline, becomes acid, as well as the rest of the mixture. It possesses this property at all periods of the experiment, as well at the ordinary temperature as at that of the bodies of the animals; only the acidity of the emulsion is produced so much more quickly and more completely as the pancreatic fluid itself is more albuminous and its temperature more elevated.

At a subsequent meeting on the 15th July 1851, M. Colin communicated an account of various experiments which he had since performed on the same subject.

These experiments, which were undertaken with the purpose of confirming his previous researches, were made upon the horse, the hog and the sheep. The author ascertained the following facts.

1. In the horse the pancreatic secretion appears to be nearly as abundant as in the cow and the bull, as was anticipated from the size and weight of this animal, as well as from the volume of the pancreas.

2. The exact appreciation of the characters of the pancreatic secretion and the quantitative determinations of its amount are almost impossible in this animal, in consequence of the difficulties inseparably connected with the establishment of a pancreatic fistula, and the disturbance which immediately follows this operation, both in the action of the gland and in the digestive organs, as well as in the economy at large.

3. The pancreatic juice of this quadruped is very fluid and very little albuminous, as MM. Leuret and Lassaigne had already shown. This peculiarity, entirely exceptional, which distinguishes it from the earliest moments of the experiment, renders its action upon fatty matters so feeble, that it is unable to produce a complete and homogeneous emulsion, however small be the proportion of oil placed in contact with it.

4. In the hog the pancreatic secretion affords generally from twelve to fifteen grammes of fluid during the earliest hours; that is to say, the seventeenth part of what is furnished by the pancreas of the horse, which is only one time more voluminous than that of the pachyderm. The fluid secreted produces a perfect emulsion of the fatty substances only when it enters at the rate of two-thirds into the composition of the mixture. But it is altered very rapidly, and even from the first moments of the experiment it is so little albuminous that it does not undergo coagulation, and becomes only slightly turbid by the action of heat.

5. In the sheep, the pancreatic juice is thick, very albuminous, and in a greater proportion, comparatively, during the first moments of experi-

ment; a circumstance on which depends its faculty of forming fatty matters into an emulsion as completely as possible.

6. In the same animal the mixture of pancreatic fluid with bile, capable of being easily collected, in the state in which it flows into the intestine, possesses the emulsive and acidifying property, which is so remarkably possessed by the first of these fluids in a state of purity. But this property presents numerous variations which depend on the quantity, more or less considerable, of the pancreatic juice in the mixture, and which correspond consequently to the oscillations and the intermissions in the secreting action of the pancreas.

7. The abundance in the pancreatic secretion of different animals is not always in proportion to the size of the individuals and the volume of their pancreas. This defect, or rather this variability, in proportion, depends very probably in a great degree on this circumstance, that the formation and establishment of the pancreatic fistula do not offer in all of these animals the same difficulties, and induce not a perturbation equally sudden and considerable in the action of the gland.

8. Lastly, the pancreatic juice, by the constancy of its alkaline character, and the uniformity of its action upon fat matters, which it always converts into an emulsion and renders acid, in this manner offers exclusively some differences, characters, and properties, which are invariable in all the animals, in which its properties and actions have been studied.

II. MEDICAL PATHOLOGY AND THERAPEUTICS.

8. *Observations upon Cretinism, its causes, and the probable means of prevention.* By M. FERRUS. (A Memoir read at the ACADEMY DE MEDICINE on the 10th and 31st December 1850. Archives Generales, Fevrier et Mars, 1851, p. 231 and 353).

For some time past the subject of Cretinism has occupied not a little of the attention of the French physicians and the Members of the academy of Medicine. In the account given of the Pathological Anatomy of Cretinism in another part of this Number, it was mentioned that M. Ferrus had been making researches upon the nature and causes of this distemper.* The following is a summary of the Memoir read by this physician in December 1850.

After stating the situation long ascertained to be occupied by individual Cretins in different countries, and having established by descriptions, that it had been agreed to distinguish in Cretinism three degrees, represented by the three terms *Cretins*, *Sub-Cretins*, and *Cretinous* persons, M Ferrus has brought out the shades which separate these different conditions and form the basis of this classification. He then directs attention to the topographical and physical characters of the districts most particularly ravaged by this distemper, and especially to his investigations in the mountains of the Valais and the Pyrenees; enumerates the alleged causes of the disease, and discusses by the method of approximation the question of the influence of waters, to which an official and public report had recently given considerable interest. On this point M. Ferrus announces the inference, that the constant presence of magnesia in articles of food and drink cannot be completely without effect in the production of Cretinism, and that the changes to which the waters are subjected in traversing cultivated districts had long appeared to him to deserve in this point of view particular attention. He deduced, however, from his own researches the conclusion, that the nature and composition of the waters, in all instances in which they were not presented in conditions too exceptional, were far from being capable of being considered as an exclusive cause, or even a very preponderating one, and that it was indispensably requisite to refer the origin of Cretinism to a simultaneous assemblage of elements capable in this concurrent manner of producing combined actions.

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* See pages 336 and 345.

After having described the constitutional alterations which are observed among the populations, in the midst of which Cretinism is developed with intensity and in a persistent mode; the more distinctly marked changes which the whole economy undergoes in individuals directly struck by the disease; lastly, the defects in organisation and the alterations in structure which particularly affect the brain and its appendages, and the arrest of developement in the osseous system;—M. Ferrus shows that anatomical examination furnishes the rational explanation of all the phenomena observed in these unfortunate persons during life, the resemblance to and the difference from Cretinism presented by other physical and mental affections with which it might possess analogies, for instance, Scrofula, Rickets, Idiocy, Imbecility, Stupidity, &c. M. Ferrus concludes that we ought not to confound Cretinism with any of these disorders, from which it presents an essential difference, both as to the character of the symptoms and the nature of the causes, and that of the material conditions disclosed by Pathological Anatomy. Notwithstanding this, though cretinism constitutes a special disease, and requires a particular position in any nosological table, M. Ferrus remarks that, definitively, Cretins ought for the most part to be associated, so far as the principles of administrative and judicial management require, with the member class of those persons who labour under mental derangement; that is, with individuals whose moral liberty is controlled or withdrawn, either in consequence of original and primary fault of organisation, or under the influence of active and permanent causes.

The author then presents in formular shape the following propositions as conclusions.

1. As *prophylactic measures, general and local,* M. Ferrus recommends works having in view to promote ventilation, irrigation, and amending the salubrity of the surface, according to the rules of public hygiene. One precaution to which, with M. Grange, M. Ferrus attaches a high degree of importance, is to collect at their source the waters deemed salubrious, and to convey them to the places at which they are to be used, by means of pipes or tubes so perfectly closed, as to prevent all communication with the soil and all filtration. To this measure the author assigns importance on a twofold account. It would not only prevent these waters, in their course through the soil and over its surface, from collecting vegetable and animal matters, but it would prevent them from becoming loaded with magnesian salts, to which this chemist ascribes considerable action, and one injurious to individuals. He next recommends the application of all the principles of private and individual hygiene, nutritious food, tonic drinks, iodized water, ferruginous water.

These first measures are to be applied at the same time, but in different proportions, to Goitrous patients and to Cretins. For the last he recommends the institution of workshops, the establishment of nurseries in elevated situations for rearing Cretin children at an early age, the employment of food different from what is obtained in the localities, and measures of similar nature.

With these hygienic measures therapeutic means ought to be combined, such as calculated by their union to furnish useful results. Thus the physician may employ within rational limits, against Cretinism, the treatment often successfully employed against Stupidity (cerebral œdema), or against incipient hydrocephalus, and which consists in powerful revellents employed both externally and upon the digestive passages. The resinous purgatives prudently administered, and especially the cutaneous revellents applied over the shaven scalp, ought in all probability to produce good results.

2. As *measures of administrative and judicial management* he recom-

mends the following. Seclusion of Cretins under the title of Alienated Idiots; restrictions imposed on their civil rights, or at least the application to this class of persons of the articles of the code relating to prohibition of marriage, as to individuals whose liberty is not complete.

3. As measures directed to the *intellectual and moral constitution* of Cretins, the following are recommended. The establishment of schools; instructions appropriated to the state of the persons; and the elements of which may be borrowed within certain limits both from what is practised as to idiot children at Bicetre, and the Salpetriere, and from the treatment employed at the special asylum of Dr Guggenbuhl.

4. As an accessary but preliminary measure, M. Ferrus recommends an exact enumeration by census of Cretins, to be subjected to the versification of the inspectors of the Service of the Insane, as a means which would indicate in the localities in which Cretinism was most prevalent, the number of persons who are attacked by the disease, and would determine as accurately as possible the degree of the disease.

The memoir of M. Ferrus was followed by a discussion which occupied three subsequent meetings. As this discussion related chiefly to the etiology of the disease, it may be useful to state the two following conclusions as embracing on this point the principal results obtained by Dr Grange.

1st. Goitre and Cretinism are owing neither to any one meteorological circumstance, nor to any one hygienic circumstance, nor to the conjunction of several of these causes, which can perform only a secondary part.

2d. Goitre and Cretinism are endemial in magnesian strata. Some goitrous patients are found upon the marine-molasse; this disorder increases upon the lias; it is general upon strata of trias, variagated marls, shell-limestone, and magnesian limestone (zechstein); it diminishes upon the strata of the coal measures, and disappears upon the granitic strata. Its greatest intensity is always observed over the great Dolomitic formations. These disorders follow upon a sufficiently great extent the alluvial deposits which proceed from the countries in which Goitre is endemial.

The opinions of M. Bouchardat, who spoke first, are represented as not baving been perfectly well understood. So far as they were understood, however, he ascribed a preponderant action to the influence of the potable waters; but when the question arose to determine the principle or the principles, the presence or absence of which give to these waters their noxious property, M. Bouchardat hesitatedt o adopt the opinion of M. Grange, and he was inclined rather to ascribe the hurtful influence to salts of lime than to magnesian salts. On the other hand, as to the opinion which admits that the waters might be hurtful, because they contained not a useful principle, as Iodine, Bromine, Iron, Oxygen, in sufficient quantity, M. Bouchardat thinks that the quantitative analyses are completely wanting to form any precise opinion on this point.

M. Bouchardat found more than one opponent. M. Caventou remarked that it was impossible to admit, without great reserve, the opinion relative to the influence of potable waters, until accurate analyses shall have shown what is understood by salubrious and insalubrious waters, in reference to the developement or the absence cf Goitre and Cretinism. M. Grange had cited indeed the fact of the development of Goitre among the inhabitants of Montmelian, who made use of well-water, and in which the goitre diminished or disappeared after they had ceased to use well-water, and substitute in its place spring-water, for drinking. But on this M. Caventou remarked that M. De Niepce had been on the spot, and that he had ascertained by analysis that this spring-water also contained a notable proportion of magnesia.

On the other hand, M. Delafond, Professor of the School of Alfort, opposed this exclusive doctrine by facts derived from comparative anatomy and pathology. It is a fact, he argued, not to be disputed, that domestic animals partake of all the conditions of the existence of man, that they experience the same influence from localities, from kinds of food, and mode of habitation, which man does. Goitre, which has been met with in some animals, and is observed in Switzerland, in dogs and horses, has no where been seen among them as an endemic disorder. Meanwhile, animals are quite otherwise exposed from man to make use of water containing in solution calcareous and magnesian salts. Around the capital, at Vincennes, all the wells, the waters of which are drank by animals, contain a considerable quantity of sulphate of lime. Even the plants on which these animals are fed, saintfoin and clover, contain this salt in large proportion. Yet these animals never contract goitre, while we observe in them the formation of large vesical concretions, formed chiefly of carbonates of lime, and the developement of calcareous phthisis, the *pommeliere*, or glandular disease, in which are sometimes found genuine deposits of calcareous salts. This, which is true of selenite or sulphate of lime is equally so of magnesia; for in animals, and particularly in horses, are found concretions and deposits formed almost exclusively of ammoniaco-magnesian phosphate, yet without goitre being observed to take place. The analogy, it must be admitted, is very remote. But it is impossible, that there can be, in this immunity from goitre enjoyed by the domestic animals, compared with the human race, any thing very extraordinary, and which it is difficult to explain, by admitting the influence of the waters used as drink as the sole and only cause of this distemper.

The nosological and pathological question of the subject was considered only by M. Rochoux and M. Baillarger. The discussion turned more particularly upon the nature of the disease and the pathological changes with which it is associated. M. Baillarger looked to the point of the analogies and the differences between Cretinism and Idiocy, comparing the differences which have been stated to subsist between these two disorders, he aimed at this result, that these differences had been imperfectly estimated, or were insufficient to distinguish these two disorders in the Nosological Table. He therefore infers the identity of the two disorders. His conclusions were verbally the following.

1. Cretinism and Idiocy are essentially characterized by an arrest of developement and of intelligence.

2. In Cretins, and in idiots, this arrest of developement must be referred to the same anatomical lesions, that is to say, to anomalies and lesions in organization.

3. Hydrocephalus is found accidentally in Idiots and in Cretins; but cannot in the present state of science serve as the foundation of a definition of Cretinism, and to be regarded as the constant and essential anatomical character of this distemper.

4. Keeping in view the differences which subsist between Idiots and Cretins, considered in a general manner, these differences can be regarded only as accessory and insufficient to distinguish Cretinism from idiocy.

5. The application of the law to prevent the marriage of Cretins would be a measure very difficult to be carried into effect, but particularly, it would be a measure inefficient to prevent the propagation of hereditary Cretinism.

6. The most practicable means probably for obtaining this result would be; 1. to determine correctly the conditions of health, which in

parents might lead competent observers to dread the developement of Cretinism in the children of any given marriage; 2. In the absence of every other method of greater efficacy, to request the active interposition, and the counsels of Medicine, in order to prevent, as far as possible, matrimonial unions between persons who present any one of these conditions known to be adequate to the production of the distemper.

M. Ferrus examined separately and successively the propositions enunciated by M. Baillarger, refuting them and maintaining the correctness of the statements advanced in his first communication. To the reporters he appeared to be more fortunate in his answer relating to the differences which subsist between Cretinism and Idiocy than in his efforts to maintain the localization of Cretinism in chronic œdematons hydrocephalus, in so far as modern researches are neither sufficiently numerous nor sufficiently complete, to enable us to decide the question of the nature of the distemper.

The discussion terminated in the adoption of two propositions. The first was to send to the Ministers of Commerce and of the Interior the Work of M. Ferrus, with the Report of the discussion to which it gave rise. The second was to name a Commission entrusted with the duty of investigating every question relating to Goitre and Cretinism, and to prepare a complete Report on the subject.

The interest excited by this discussion in the academy has induced several physicians settled in the territories in which Cretinism and Goitre are endemic to communicate their observations to the body. These observations are not, in general, favourable to the opinions defended by M. Grange. M. Le Dr Niepce, for instance, physician to the sulphurous waters of Allevard, who has been occupied for three years previous to 1851 in making researches upon this subject in the valleys of the French Alps, of Savoy, Piedmont, and the Po, has seen reason to conclude that the causes of Goitre and Cretinism are manifold. He has made numerous analyses of the water of the vallies around Grenoble, where are many Cretins and goitrous persons, and he finds that these waters contain no magnesia. M. Niepce has, in like manner, analysed a great number of springs in the department of the High Alps, in which are many goitrous persons; these analyses have given the same results. M. Aguilhon of Riom, in Auvergne, who has made researches on the disease in the department of Puy-de-Dome, where are observed rare and isolated instances of Cretinism, while goitres are in the district generally numerous, and in the endemic state in several valleys, arrives at the conclusion, that the knowledge of the waters and the strata and soils, general observation and statistical inquiries, all concur to controvert the opinion delivered by M. Grange regarding the operation of magnesian limestone and magnesian salts upon the production of Cretinism and goitre.

Lastly, numerical returns further show that goitre has sensibly diminished in frequency in those localities in which civilisation has most extended its influence.

M. Bonjean of Chambery has, on his side, confirmed the statements which have been made by M.M. Grange, Ferrus, and Bouchardat, in relation to the existence in the Maurienne of springs which appear to induce goitre in those who use these waters as drink. Of these springs M. Bonjean mentions three,—one at Montournier, one at Villard-Clement, and the third at Leyssaud. But the analysis which he has made of the tuffs or deposits formed by the waters have given only very small proportions of sulphate of lime. The magnesian salts are in these deposits in equally small proportions, especially in the deposits of Villard-Clement and of Leyssaud. The first are distinguished by a considerable proportion of silica.

9. *The Spontaneous Development of Air in the Blood as a cause of Death.* By M. Dr Durand Fardel, (Archives Generales, Proceedings of Academy of Medicine, January 1852. P. 98.)

A lady, aged fifty-six, of tall stature, and considerable corpulence, came to Vichy, accompanied by her husband who was labouring under gravel. This lady in apparent good health, complained sometimes not of palpitations at the heart, but of a little difficulty in breathing, and wished like many persons to take baths, for which purpose she obtained the authoritative sanction of the physician, under whom was her husband.

On the 20th of July she went to the bath-establishment to take her second bath, at eight o'clock in the morning. She was well the previous evening, had dined in the usual manner, and had slept well. The only uneasy feeling which she had was shortness in breathing; she was obliged to remain seated before she entered the bath. Half an hour, she requested to be taken out. She was in a bad condition; and when she rose from the bath to change linen, she appeared to be in agitation, and complained of oppression. She then went out and sunk down on a seat, with panting breathing, and unable to speak. When M. Durand came five minutes after, she was lifeless. The face was quite void of colour; the head fallen back on the chest and moveable; the lips blue; the features calm and undisturbed, without foaming at the lips; the limbs flaccid and insensible. The pulse had ceased at the wrist and heart. The pupils were dilated and moveable,* the conjunctivæ insensible to touch.

Although these phenomena left no doubt upon the cessation of life, the median basilic vein was largely opened, and there flowed from it a little blood, not black, but violet-coloured, frothy; that is to say, accompanied with air-bells of unequal size, which at the same time escaped from the vein. For more than fifteen minutes, M. Durand employed unavailing attempts, tickling the uvula, presenting ammonia to the pituitary membrane, &c. During this time the frothy blood continued to trickle from time to time under the influence of pressure made on the fore-arm from below upwards. Once the jet was discharged forcibly, and continued for five or six seconds, as if it had been propelled by a bell of air which might be expanded in the interior of the vessel. A small quantity of white foam then apperead on the lips.

The body inspected twenty-two hours after death presented the following phenomena.

No trace of decomposition; some discoloured marks only upon the dependent parts of the trunk and extremities.

The heart was very much enlarged. The right chambers were distended by blood entirely liquid, violet-coloured rather than blackish, as if syrupy, very frothy; the bells of air which it contained were some of them very numerous, of the size of pin-heads, others fewer of the size of garden-peas.

When pressure was made upon the course of the two *Venae Cavae,* the blood which flowed into the right auricle was frothy like soap water. The walls of the right chambers of the heart presented superficially a violet-coloured staining. The left chambers were quite empty of blood, and void of colour. The left ventricle was considerably hypertrophied. No change had taken place in the orifices of the heart, nor at the begimning of the aorta.

The lungs filled the chest and presented a small number of adhesions, without any trace of emphysema. Their colour was a little reddish externally, deeper coloured in the interior, where they presented traces of bloody congestion to a considerable degree, without infiltration of blood. There was frothy congestion moderately abundant in the depending portions. A little whitish frothy mucus was found in the bronchial tubes.

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* For moveable we suspect it ought to be motionless.

The whole abdominal venous system was gorged with violet-coloured frothy blood, and numerous bells of air were observed in the blood of the splenic vein and portal vein.

In the abdominal organs, there was bloody congestion to a considerable degree in the liver, the spleen, the kidneys, and injection of the veins of the omentum and mesentery. The omentum was much loaded with fat.

The stomach was large and voluminous, containing about half a glass of clear colourless mucus. The intestines were not opened. The gall bladder contained a moderate amount of blackish syrup looking bile.

In the brain was the same degree of bloody congestion as in the other organs; a little liquid blood, not frothy, was found in the sinuses of the *dura mater*. The brain and origin of the spinal chord were a little injected with blood. No air was observed in the vessels.

M. Durand-Fardel thinks, that in this case we must admit the spontaneous volution of gas during life in the venous system, probably in consequence of a spontaneous alteration in this fluid, neither the nature nor the cause of which, however, is known.

10. *Caustic-Holders made of Gutta-Percha.* (In a letter to the Editor.)

Sir,—It is well known that the sticks of nitrate of silver corrode, blacken or injure the various kinds of caustic-holders, except those made of silver; these last are expensive, and after some time, even they are somewhat injured.

I have of late employed gutta-percha for this purpose; a piece of a "hearing-tube" may be used. Gutta-percha being incorrodible and cheap, seems better adapted than any other substance for this object.

If I am correct in my supposition that this proposal has not been previously made, your insertion of this note will much oblige.—Yours, &c.,

Prince-Edward Island, Thomas Stratton, M.D., R.N.
1st March 1852.

III MEDICAL SURGERY.

11. *Instance of Foreign Body impacted in the Orifice of the third Branch of the Right Bronchus, with Remarks.* By John Gregory Forbes, Fellow of the Royal College of Surgeons, England. (Medico Chirurgical Transactions. Volume xxxiii., p. 1. London 1850.)

In the forty-second volume of this Journal, was given a series of cases illustrating the nature and effects of the accidental introduction of Foreign Bodies through the Larynx into the Wind Pipe and Bronchial Tubes; with references to other cases either in the same journal or in other writings. One of the objects of the writer was to correct what he thought he perceived to be certain erroneous notions prevalent upon the situation of these bodies: namely, that they are situate not in the lungs, properly so called, but in one or other of the upper bronchial tubes upon the right side; that in this situation they give rise to symptoms, in some instances very intense, of chronic *bronchitis*, so intense indeed as completely to imitate the symptoms of pulmonary consumption, and to have been mistaken for that disease; that, nevertheless, under certain favourable circumstances of shape, figure, and weight of the body so conveyed into the *bronchi*, it may at length be expelled by coughing;

and the individual may make a recovery temporary and partial, or permanent and complete.*

The nature of this species of bronchial inflammation was considered in the first edition of the Pathological Anatomy of Dr Craigie, and in the Practice of Medicine of the same author; and in the second edition of the Pathological Anatomy was given a tabular view of twenty-four well authenticated cases of the incident, and the inferences which these cases seem to warrant.†

In the inference, number eight, it is stated that "In certain cases, in which the mechanical configuration of the body is unfavourable for detachment and expulsion, the bronchitic symptoms are liable te be extended to the lung, in which suppuration is caused, and to the pleura, in which effusion of lymph and purulent matter is introduced."

This is what takes place in the most intense and long continued cases of the incident, whether the body is expelled and recovery is effected, or the body remains and death ensues.

Besides the instances recorded in the works referred to of this extreme form of the lesion, a pointed example is recorded by Mr W. G. Carpenter, in Guy's Hospital Reports, (volume seventh, p. 353. London, October, 1842), and of which an abstract is given in the seventy-first volume of this Journal p. 234, January, 1849.

An example of the unfavourable form of the incident is recorded by Mr Forbes; and it has induced this gentleman to consider carefully the circumstances indicating and contraindicating the operation of opening the windpipe.

The inquiry involves not merely the question of operation and its admissibility. The operation is in general perfectly practicable, so far as making an opening into the wind-pipe is concerned. But it by no means follows that when the opening is made, the body can be extracted or is removable. Thus in the noted case of Sir Isidore Brunell, in whom the operation was performed for the removal of a coin, it was found impracticable to extract the coin by forceps; but it was afterwards expelled by coughing, or rather by the effect of gravitation. In most of the cases hitherto tried, the introduction of the forceps is followed by violent coughing, and the appearance of suffocation so threatening, that it is impossible to persevere. In certain cases, however, though the body cannot be reached or seized by the forceps, it is eventually expelled through the artificial opening.

The following are the details of the case given by Mr Forbes, in which it was considered unsafe or inexpedient to attempt the operation.

Mrs W., aged 46, of pale complexion and thin person, applied at the Western General Dispensary on the 11th of May 1849, stating that on the previous day, at 2 P. M., whilst eating some broth, a piece of solid matter, which she believed to have been 'bone covered with gristle,' passed into the windpipe. She was immediately seized with spasmodic cough and threatened suffocation; 'her face became black, and water ran from her mouth,' and it was some minutes before she recovered herself. She fancied that at first she felt it sticking across the windpipe, but that she squeezed it lower down with her fingers. Soon after the accident happened, a probang was passed into the stomach, but no obstruction was found.

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* Edinburgh Medical and Surgical Journal, vol. xlii., p. 103. Edinburgh 1834.

† Craigie's Pathological Anatomy, 2d edition, p. 590. Edinburgh and London, 1848.

The following was her state at 2 P. M. on the 11th of May.—Her voice was hoarse, the respiratory movements were slow (not averaging more than 10 or 12 per minute), and accompanied with a wheezing noise; and she was distressed by a constant short cough, aggravated by full inspiration. There was pain at the upper part of the chest, which she referred to the junction of the second rib with the sternum, and at the back of the neck. Pulse 84; tongue rather furred. The expression was somewhat anxious, but there was no lividity or suffusion of the face. The symptoms were increased by exertion, or by lying on the right side. There had been no shivering, nor any return of the suffocative cough.

On auscultating the chest, a marked difference was found to exist between the two sides. On the right, the breath-sound was obscured, the natural vesicular murmur being scarcely perceptible, and a 'prolonged and peculiar rhonchus' was heard throughout the lung, but most distinctly over the point to which the pain was referred, and was more audibly marked during expiration. On the left side the respiratory sounds were feeble, but free from rhonchus, and both inspiration and expiration were lengthened.

An inquiry into the previous state of her health elicited that she had been subject to 'pain in the right side, and to tightness in the chest;' but that she had never suffered from cough or expectoration.

These symptoms, and the previous history of the case, led the colleagues of Mr Forbes, Dr M'Intyre, Dr Hennen, and Dr Miller, to agree with him in opinion as to the presence of a foreign body in the air-passages, and that its probable situation was in or near the right bronchus. Eight leeches were applied to the chest, and a saline mixture with antimony ordered to be taken at intervals.

On the 12th, the report was that she had passed a restless night from constant hacking, dry cough, which was immediately brought on by any effort. The pain had been relieved by the leeches, which bled freely. She had no sickness or dysphagia.

On carefully examining the chest, the right side was observed to rise less during inspiration than the left; the respiratory sounds were audible, though still obscured; and posteriorly there was some dulness on percussion, whilst under the clavicle the usual clear sound was elicited. Some wheezing sounds were present, and expiration was attended with a 'snoring rhonchus,' which varied somewhat in character at different times.

In the evening she had a severe shivering fit, which lasted an hour and a half, followed by heat of skin and pain in the back. At half-past seven the pulse was 114, easily compressed. The tongue was dryish, and she complained of some thirst. The sound with expiration had now more of a 'cooing' character.

13th. She slept three hours in the night. Slight expectoration of frothy mucus free from odour, and not tinged by blood. Complains of some pricking pains under the left clavicle, and in the right hypochondrium. Pulse 96. Skin cool. No lividity of countenance. She is sensible that the 'cooing sound' proceeds from the right side of the chest, and she makes continual efforts, by coughing, to remove the bone, which she believes to be situated in the same situation to which she first referred the pain. She was threatened with another shivering fit in the evening, but kept it off by hot bottles applied to the feet. Some tincture of hyoscyamus was substituted for the antimony in her saline mixture.

14th. Slept three hours, but awoke with tickling in the throat, owing, she thinks, to accumulation of mucus. The cough has been incessant, with mucous expectoration. Pulse 108. The auscultatory signs the same. In the afternoon she was visited by Dr M'Intyre, Mr Arnott, Mr Anderson, and myself. We then found that since the morning she had been more tranquil, that the wheezing and dyspnœa were less, though her respirations were 40 per minute, that the expectoration was moderate, and, upon the whole, that she was less distressed. This favourable change led to the supposition that the foreign body might have somewhat altered its position, so as to cause less irritation, and to offer less obstruction to the respiration. On the following grounds it was agreed that the operation should not then be performed, though the possibility of its becoming necessary at a future time, from the existence of more urgent symptoms, was admitted.

1st. There was a doubt as to the nature of the offending substance. If gristle, it might be softened and coughed up, and would necessarily give rise to less irritation and mischief than bone.

2dly. The clear sound on percussion under the clavicle, and the fact of respiratory sounds being heard there, did not indicate any great amount of obstruction to the entrance of air into the lung: and—

3dly. Though a peculiar rhonchus was heard over the right bronchus, its weight, as an adverse symptom, was materially diminished by the comparatively free respiration in the upper part of the lung. (This apparent inconsistency was fully explained by the position of the foreign body as discovered after death.)

On the 16th, the "cooing sound" had degenerated into a peculiar "whiff" or 'puff,' heard as before, during expiration. The expectoration was more copious and diffluent, though still mucous and frothy. The cough was at once brought on by any exertion or slight excitement, such as would be caused by the unexpected entrance of a person into the room. Fearful of the effects of this continued irritation on the structure of the lung, and perhaps on the patient's life, Mr Arnott's assistance was again sought; and on the 17th, he met Dr Miller, Mr Anderson, and myself in consultation. Owing, however, to the uncertainty which was felt in fixing on the exact position of the foreign body, and the consequent doubt as to the possibility of being able to grasp it in the forceps, and the symptoms not being of that urgent nature which called for immediate interference, it was considered inexpedient to undertake an operation.

On the 18th, I found her in the afternoon complaining of much pain in the throat and 'all over her,' with the skin hot, the pulse 132, and the respirations 48 per minute. A blister, which had been applied the day before under the right clavicle, had given her some relief.

A mixture containing acetate of ammonia, and decoction of senega, with tincture of hyoscyamus was prescribed.

From the 18th of May to the 1st of June, the symptoms were, with little variety, restless nights, paroxyms of fever, occurring almost daily, generally in the afternoon; profuse night sweats, general pains, with severe cough, and expectorating of muco-purulent matter. The respirations varied from 36 to 40 in the minute; the pulse from 96 to 120. The urine was high-coloured and loaded with lithates. The bowels in general acted without medicine. The expiratory blowing continued.

The left lung appeared to be free from disease.

On the 1st of June the symptoms were aggravated; the expectoration was increased, amounting to a cupful in the course of the day; it was mucous and frothy.

The decoction of senega was given with dilute sulphuric acid, syrup of poppies, and syrup of tolu.

Between the 1st and 21st of June, the symptoms underwent some remission. On the 8th, the pulse was 80; fever was abated; and the patient sat up in bed and did some work. On the 17th, a little wine was allowed, and quinine was prescribed.

After the 21st, however, the symptoms became more intense. On the 25th, the pulse was 136, respiration 44 in the minute. The other symptoms of cough, breathlessness, and expectoration, with intense advancing weakness unabated. The chief auscultatory signs were loud mucous rattles, and dulness on percussion over the right side of the chest, with blowing respiration in parts. These were, however, less marked under the right clavicle, the air appeared to enter with more freedom into the upper part of the lung than elsewhere.

In this state she continued till the 5th of July, when Mr F. found her lying in the horoizontal posture in her bed, unable to speak, and death, which had been daily expected for a week, took place at midnight.

Sectio Cadaveris, 16 hours after death.—Present Messrs Arnott, Shaw, De Morgan, and Anderson, Dr Miller, Mr Palmer, and Mr Forbes. The following report is drawn up from notes taken at the time by Dr Miller, and a drawing by Mr William Wing gives a correct representation of the appearances observed.*

Body much emaciated. Decomposition commencing. The intercostal muscles in front of the chest on the right side were much darkened, and contrasted strongly with those on the opposite side, which were of the usual florid colour. On exposing the trachea and great vessels in the neck, the right carotid artery was observed to pursue an abnormal course, running obliquely upwards and to the right side in front of the trachea, from a point a little to the right of the left sterno-clavicular articulation; when the sternum was removed, this was found to depend on the innominata artery arising somewhat to the left of its usual point of origin, and taking a more perpendicular course than it ordinarily does, and dividing early into its two branches. The distance from the lower border of the thyroid body to the upper edge of the carotid artery, taken upon a line drawn perpendicularly through the centre of the trachea, was one inch, and this was increased half an inch by continuing the measurement to the top of the sternum. Both the arch of the aorta and the innominata artery appeared to bulge more than usual.

The right lung was seen well filling its own side of the chest, and in its inferior two thirds was adherent to the ribs and diaphragm respectively; and close upon the latter in front, there was a pleuritic abscess, the size of the palm of the hand. The left lung appeared somewhat collapsed.

The veins were now tied and divided, and with the large arteries turned back, so as to expose the anterior surface of the trachea and bronchi. The course of the treachea and right bronchus was an oblique curve, deviating but little from the perpendicular, bearing from the upper border of the sternum downwards to the right of the middle line. The left bronchus went off more abruptly. The trachea and right bronchus were now slit up in their whole extent, and found to contain a thin mucopurulent secretion of a pale slate colour. In the latter, at the distance of one inch and a half from the point of bifurcation of the trachea, and five inches and a half from the lower border of the thyroid body was found a small piece of bone, weighing, when dry, three grains and a half, having a concave smooth facet, and a convex rough one, and one very sharp edge, its breadth being three-eighths, and its length a quarter of an inch. It was firmly impacted in the orifice of the third branch, given off from the bronchus, which passed into the middle lobe,† and this accounted for the comparative freedom with which the air appeared to enter the upper part of the lung throughout the case, as evinced by the auscultatory signs. The mucous membrane around it was of a vivid red colour, and highly injected, but gradually became paler towards the left bronchus, where, as well as in the trachea, it presented its usual colour.

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* The preparation is deposited in the Museum of the Royal College of Surgeons.

† The course of this branch of the right bronchus is marked in the drawing by a bristle inserted into it.

The lower two thirds of the right lung were of an ashy-slate colour, of dense consistence, very offensive odour, and infiltrated with purulent fluid. Small portions of it sunk in water, and when washed, it had much the appearance of coarse dark sponge, though no distinct cavities containing pus were visible.

That part of the upper lobe which was not adherent to the side of the chest and which was supplied with air by the first branch of the right bronchus, containing air, and the upper portion of it appeared healthy.

The left lung contained no trace of tubercle, and was perfectly healthy.

The heart was large, and slightly distended to the left.

The liver was large, but healthy in structure, and extended into the left hypochondriac region.

Some other observations follow as to operation, for which the original may be consulted.

IV.—MEDICAL POLICE.

12. QUARANTINE LAWS, *as Means of Preventing the Importation and Propagation of Yellow Fever.*

We have been requested by the Chairman and Secretaries of a Committee of the Metropolitan Sanitary Association to give insertion to the following Report on a Work or Memoir by Dr James Gillkrest on the question of the Contagious or Non-Contagious character of Yellow Fever, and on the Utility or Inutility of Quarantine Laws.

Translation of a Report by MM. Magendie, Louis, and Londe (Reporter) to the French National Academy of Medicine, on a Work by James GILLKREST, Esq., M.D., &c., Inspector-General of Army Hospitals, entitled, "*Is Yellow Fever Contagious or not?*"

"You have charged MM. Magendie, Louis, and myself, to render an account of a work by Dr James Gillkrest, Inspector-General of Army Hospitals, in the service of Great Britain; which is entitled, '*Is Yellow Fever Contagious or not?*'

"The history of Yellow Fever is treated by M. Gillkrest with an erudition which it would be difficult to find equally complete elsewhere. After mentioning observers and monographers, describers of yellow fever, beginning with Hippocrates, who speaks of a fever charaeterized by yellowness of skin, and black vomit, M. Gillkrest arrives at these conclusions.

"1st, That the yellow fever of America, and that observed on the south-west of Europe, especially in Spain, are identical; an identity acknowledged by all authors, with the exception perhaps of our colleague M. Rochoux.

"2dly. That this disease existed in the Antilles before 1793, and in the Spanish Peninsula before 1764.

"M. Gillkrest next relates a great number of facts establishing:—

"1st, That yellow fever, or at least its pathognomonic symptoms, have shown themselves at very remote points of the globe, and that they appear to be then developed uniformly under accidental or local conditions, so strongly marked as to exclude all idea of importation in the true meaning of that word.

"2dly, That sporadic cases of yellow fever present themselves, in ordinary years, in the localities where the disease has prevailed in an epidemic form. M. Gillkrest considers that this second inference is established by a certificate by the medical officers, who, on the 13th of April, 1829, at Gibraltar, declared,—after having read with the greatest care thirty-nine cases extracted from the records of the Civil Hospital,—that the symptoms detailed in these cases were perfectly identical with those observed in the epidemic which prevailed in that garrison in the latter part of the year 1828.

"Having established these fundamental points, M. Gillkrest arrives at the grand question, contagion.

"The author establishes, by numerous well-selected and incontrovertible proofs, that yellow fever is not contagious under any circumstances, not even in the case of crowding in this disease, whether of the dead or of the living; that the removal of the individual from the influence of the local causes which produce this affection is the fittest means of preventing its extension; and, lastly, that the cordons, called sanitary and quarantine measures, far from arresting yellow fever, on the contrary favour, its extension by confining the population within the influence of the local causes which gave it birth.

"Such, gentlemen, is the work on which we have to report. M. Gillkrest, in crowning services which do him honour, and which on more than one occasion have been extended to our fellow-countrymen, brings to bear upon the question of contagion in yellow fever, (a question so intimately connected with the most important interests of mankind,) the fruits of a long experience, as was done formerly by our intrepid and ever to be regretted Chervin.

"This communication of M. Gillkrest, which has already received the full approval of the General Board of Health in London, has reached us most opportunely at the moment when a congress is assembled to suggest, no doubt, important modifications in our sanitary laws; consequently, gentlemen, your committee has the honour to propose:—

"1stly, To thank the Honourable M. Gillkrest for his interesting communication.

"2dly, To transmit his work to M. the Minister of Commerce, in order that in conjunction with the numerous documents on this subject, already in possession of the administration, this work may aid in placing beyond a doubt the inutility of quarantine, as applied to arrivals from countries where yellow fever prevails.

"Those resolutions were put to the vote, and adopted."

Bulletin of the National Academy of Medicine,
Vol. xvii. No. 2, 31 Oct., 1851, III. p. 39.

In inserting the above report, and the conclusions therein contained, we feel it requisite to observe, that they contain nothing new to the readers of this Journal. The question as to the origin and alleged contagious propagation of yellow fever have been fully discussed and considered in various articles in many volumes of this Journal; and we think that we claim no more than ordinary and just merit for these articles and the facts and arguments which they contain, when we say that they have exhausted the subject, so far as the adduction of ordinary historical facts and reasoning can go in deciding the point at issue.

The spontaneous, telluric, or atmospheric origin of this disease has been proved with as great certainty as the subject admits, in volumes nineteenth, twenty-first, twenty-ninth, thirty-fourth, and thirty-fifth; and the original source of the fancy that it was created at Bulam on the west coast of Africa, and thence imported into the Caribbean Islands, was fully explained and considered in the thirty-fifth volume of this Journal; Edinburgh, 1831.

It is possible, nevertheless, that the above Report, proceeding as it does from a body of well-informed scientific persons, who ought to examine the question in all its aspects and all its bearings in a dispassionate and unprejudiced manner, ought to have more weight than any inferences proceeding from individuals, however justly and carefully deduced. On this account it is to be hoped, that giving the report such a degree of publicity as we can, may have beneficial effects.

On the other hand, there are, relating to this and all questions placed in the same circumstances, difficulties almost insurmountable to any good results being obtained. The administration of the laws relating to quarantine is entrusted to a set of persons, whose ignorance can be equalled by nothing but their obstinacy. To these persons offices of rank, authority, and considerable emolument are assigned; and, indeed, their authority rests upon, and their emoluments are derived from, maintaining the opinion and the principle, that yellow fever is a contagious disease, and can be imported from the West Indies and the south of Spain. So long as these gentlemen are in the receipt of large sums of money for maintaining these opinions, it is altogether unreasonable to expect, that they will ever relinquish these opinions, or be made to admit that yellow fever is not contagious, and cannot be imported. To expect such a concession, is to require too much from such frail materials, as those of which men and physicians are composed. We, therefore, regard the whole matter of controversy as nearly altogether useless and unprofitable. Those who can be convinced, have not the power to abrogate or alter the quarantine laws; and those whose interest it is to maintain the present state of things never will be convinced. We believe, therefore, that a long time of ignorance, presumption, and conceit, as well as avarice, must yet elapse, before any reports from scientific bodies shall be of essential service.

13. *Establishment of a Lectureship on Military Surgery in Dublin.*

Directions of H.E.I.C., that candidates for admission into their service shall attend Lectures on Military Surgery.

In the fortieth volume of this Journal, (p. 456), in giving a view of the History of Military Medicine and Hygiene, we were led to observe that the chair of Military Surgery in Edinburgh, which has been the earliest establishment of the kind in Europe, may become the model of similar establishments in different countries. It is scarcely necessary to explain, that this was said in reference to the duties of the chair being chiefly directed to the Department of Military Medicine and Hygiene in general, as elucidated in the article in that volume. A long time may perhaps elapse before this prediction may be fully realized; and so far as it is connected with the exigencies of a state of war, we have little desire to see it realized at all, or in any way.

War, however, it is remarked, in the same article, is a necessary evil; and in the present state of the world, it is not more likely to be dispensed with than at any former period. It becomes therefore both nations and individuals to be prepared against its unavoidable casualities and calamities. If one institution be more necessary than another, it is that, the object of which is to diminish the number and mitigate the severity of the various evils necessarily attendant on those engaged in the duties of the Naval and Military service.

The most important and serviceable manner in which the Medical Officers of the army and navy can be employed, is in understanding perfectly and performing diligently, the duty of preserving the health of the men committed to their professional charge. It is both easier and more economical to preserve health, whether among individuals or bodies of men, than to restore it when impaired; and prophylactic or preventive medicine (*Medicina Phthastica*) is, though little understood, by far the most useful division of the art. This department, therefore, which allies itself with the general movement for sanitary improvements and civil hygiene, ought to obtain that serious attention, to which it is most justly entitled, in any course of instruction prescribed by public boards for public medical officers. We must accordingly be permitted to maintain, as strongly as ever, the necessity of instructing those who are to have the medical charge of soldiers and seamen, in all those branches of knowledge, the object of which is to preserve health, and to repel and prevent the attacks of disease, especially when epidemic or endemic, or in any mode dependent upon the operation of preventible causes. Our military and naval history is full of records of ruinous and discreditable losses of men, from ignorance of, and inattention to, the known rules of public Hygiene. An encampment or bivouac made in an improper and unhealthy position, an hospital erected in a bad situation, and without adequate regard to ventilation, have in too many instances caused more mortality than the fire and sword of the enemy.

All competent judges will admit, that it is a movement in the proper direction, that a Lectureship of Military Surgery, is established in the City of Dublin; and that attendance on the Course is recommended in the statutes promulgated by the University in November 1851. The only modification we should desire to see, for reasons fully stated before, is, that it be a Lectureship of Military Medicine and Hygiene. But in the meantime the act is a sort of initial step of what ought to be done with the purposes already stated. Nor is it of less consequence, that the Hon. East India Company have evinced a desire, that the persons employed as their medical officers shall have devoted attention to this branch of medical education. Certainly it would be difficult to name any place or any situation, in which such knowledge must be more requisite and more beneficial, than in those extensive regions, which are subject to the sway of the Honourable Company. We only add that this measure may probably lead to the establishment of a Lectureship in London; and whether this takes place or not for the surgeons of the E.I.Cy's. service, we think that the time is approaching when public opinion will require that medical practitioners shall study the subject of Hygiene with sufficient attention, to enable them to give sound advice on the preservation of health and the prevention of the causes of sickliness.

University of Dublin.—Diploma in Surgery.—Resolved by the Provost and Senior Fellows:—That a diploma in surgery be given to such students as are matriculated in medicine, and have completed at least one year in arts, on the following conditions:

1. To complete one year in arts, it shall be necessary to have answered at least one examination, subsequent to the Junior Freshman year; or to have completed the Junior Freshman year only, by passing the Michaelmas examination of that year, and keeping one previous Term, either by lectures or by examination.

2. Students who have not passed an examination in the Senior Freshman year will be required to attend one course of lectures in logic.

Students who have not passed the junior sophister year of the undergraduate course will be required to attend one course of lectures on mechanics, with the assistant to the Professor of natural philosophy.

3. Students so qualified will be admitted to examination for the diploma in surgery, as soon as they shall have completed the prescribed curriculum.

4. This curriculum shall extend over a period of four years, and shall comprise attendance upon the following courses of lectures in the school of physic in Ireland:

Anatomy and Physiology, - - - -	Three Courses.
Demonstrations and Dissections, - -	Three Courses.
Theory and Practice of Surgery, - -	Three Courses.
Practice of Medicine, - - - - - -	One Course.
Chemistry, - - - - - - - - - -	One Course.
Materia Medica, - - - - - - - -	One Course.

Midwifery,	One Course.
Practical Chemistry,	One Couse each, of three months' duration.
Botany,	
Medical Jurisprudence,	

[Four of the above-named courses, together with one course of demonstrations and dissections, may be attended in any school of medicine recognised by the Board.]

Also attendance for three sessions, each of nine months' duration, on the practice of any of the following hospitals, together with attendance on the clinical lectures on medicine and surgery there delivered:

1. Richmond, Whitworth, and Hardwicke Hospitals.
2. Steevens' Hospital.
3. Meath Hospital.
4. Jervis-street Infirmary.
5. City of Dublin Hospital.
6, Mercer's Hospital.
7. St Vincent's Hospital.

Of the courses of lectures which are of six months' duration, not more than three can be attended during any one session

5. Candidates for the diploma who have complied with the foregoing regulations must pass an examination before a Court of examiners, consisting of the Regius Professor of physic, the Professors of anatomy, surgery, chemistry, midwifery, and botany.

The examination of each candidate will be divided into two parts, one of which shall be devoted to anatomy and physiology, surgical anatomy, the theory and practice of surgery, and operative surgery; and the other to practice of medicine, midwifery, chemistry, materia medica, and toxicology.

6. Candidates for the diploma must submit their certificates and testimonials of qualification to the Regius Professor of Physic and to the Professor of Surgery, who shall sign the chart necessary to be laid before the senior lecturer and registrar, previous to the issuing of the *liceat ad examinandum* to the Professors.

November 1851.

This diploma is recognised by the heads of the medical departments of the army, navy, and ordnance.

The following courses of lectures and of clinical study are recommended to students intending to qualify for the public service in the above departments:

1. Ophthalmic Surgery.
2. Military Surgery.
3. Pathological Anatomy.
4. Comparative Anatomy and Natural History.
5. Attendance in an Hospital for the Treatment of the Insane.

Regulations for the Admission of Medical Gentlemen into the East India Company's Service as Assistant Surgeons for India.

Age.—The assistant surgeon must not be under twenty-two years, in proof of which he must produce an extract from the register of the parish in which he was born, or his own declaration, and other certificates, agreeably to forms to be obtained in the Office for Cadets and Assistant Surgeons, East-India House.

Qualification in Surgery.—The Assistant Surgeon, upon receiving a nomination, will be furnished with a letter to the Court of Examiners of the Royal College of Surgeons, to be examined in surgery, and their certificate will be deemed a satisfactory testimonial of his qualification: but should the assistant surgeon be previously in possession of a diploma from the Royal College of Surgeons of London, or of the Colleges of Surgeons of Dublin or Edinburgh, or of the College and University of Glasgow, or of the Faculty of Physicians and Surgeons of Glasgow, either of them will be deemed satisfactory as to his knowledge of surgery, without any further examination. He is also required to produce a certificate, from the cupper of a public hospital in London, of having acquired, and being capable of practising with proper dexterity, the art of cupping.

Qualification in Physic.—The Assistant Surgeon will also be required to pass an examination by the Company's examining physician in the practice of physic, in which examination will be included as much anatomy and physiology as is necessary for understanding the causes and treatment of internal diseases, as well as the art of prescribing and compounding medicines; and Dr Scott will then require him to produce satisfactory proof of his having attended at least two courses of lectures on the practice of physic; and, above all, that he should produce a certificate of having attended diligently the practice and clinical instruction of the physicians at some general *hospital* in London for six months; or at some general hospital in the country (within the United Kingdom) for six months, provided such provincial hospital contain at least, on an average, one hundred in-patients, and have attached to it a regular establishment of physicians as well as surgeons. It is also expected that the assistant surgeon shall produce a certificate of having diligently attended, for at least three months, the practical instruction given at one of the asylums for the treatment of the *insane*, and at one of the institutions or wards of an hospital especially dedicated to the treatment of *ophthalmic* disease. He will also be required to attend a course of lectures on the principles and practice of *military surgery*, if such a course shall be given at the place at which he has been educated. No attendance on the practice of a physician at any *dispensary* will be admitted.

The assistant surgeon is also required, as a condition to his appointment, to subscribe to the Military or Medical and Medical Retiring Fund at his respective presidency, and also to the Military Orphan Society, if appointed to Bengal.

The assistant surgeon is required by resolution of Court of the 21st May 1828, to apply at the Office for Cadets and Assistant Surgeons for his orders for embarkation, and actually proceed under such orders within three months from the date of being passed and sworn before the committee for passing military appointments; he will then be furnished with an order to obtain the certificate of his appointment, signed by the secretary, for which he will pay a fee of L.5 in the secretary's office.

14. In the Foreign Papers is announced the death of Vincent Priessnitz, at Graefenberg, on Saturday, the 29th November 1851, at the age of fifty-two years.

He had been unwell for some little time, though on the nature of his ailments no exact information is given, or could be obtained, in consequence of his rooted dislike to the regular practitioners of medicine, to none of whom he would apply. On the morning of the 29th November, Priessnitz rose at an early hour, as was his custom ; and was preparing to go through some of his duties. He felt extremely cold, and, according to the account, either gave directions for a fire, or allowed one to be prepared. Some of his friends suggested that he should send for a physician ; but he replied that it was of no use, yet gave a sort of assent. The sensation of coldness continued to increase, and Priessnitz breathed his last on the afternoon of the same day.

He appears not to have been visited by any physician. Some rumour there was of his having symptoms of hydrothorax, or some similar disease of the thoracic viscera. But upon this point nothing accurate is known.

In this manner has died, at an age comparatively early, a man who professed to be able to give health to thousands, and so far to repel the approaches of disease, that many believed, that he had added considerably to the usual resources of the art of prolonging life. It would be neither, according to the rules of good taste, nor of Christian practice, to apply to the deceased, the saying of *Tu Quoque*; or, in language still more impressive, to say, Art thou also come weak as we? art thou become like unto us? Yet it is scarcely possible for the mind of the most moderate reflecting powers, to refrain from contrasting the end of Vincent Priessnitz with the greatness and confidence of his professions as to his power of controlling disease and re-establishing health.

Vincent Priessnitz has the credit of having revived a practice which had fallen into desuetude, because it was found always difficult, and sometimes impracticable, in application. It has been already shewn in this Journal, vol. lviii., that the practice was not new; that it had been employed by Vander Heyden of Ghent, Sir John Floyer, Dr Baynard, Dr Hancock, Cyrillo, and many others. These men, as well as Dr Jackson and Dr Currie, employed the external use of cold water in a cautious and discriminating manner. Yet the method never became general, because such methods require the constant supervision of the physician who prescribes them, and the observance of different conditions in different cases and classes of cases. Priessnitz had in some sense accidentally stumbled upon the method; and not being encumbered with either doubts or scruples, he employed it in many instances indiscriminately, and in not a few hurtfully. His example, nevertheless, may lead to the means of ascertaining more correctly than heretofore the conditions for the favourable application of a therapeutic method of undoubted power, and the correct discrimination of those cases and conditions of the system in which it promises rationally to produce beneficial effects.

The foregoing notice was intended for insertion in last number (January 1852), but was postponed for want of space. Since that time there has appeared in the Archives Generales a notice, not particularly accurate, of the event, with some observations on the Character and Practice of the deceased. With some strong commendation, there are mingled not a few remarks more harsh than anything that has been elsewhere said. It is stated, that to Priessnitz belongs the honour of having given the first impulse to the hydro-therapeutic method. This is merely a figure of speech altogether at variance with the facts. His early cures, it is said, were characterized by *brutalité;* but from this, it is added, he had latterly departed, in consequence of good sense, relations with physicians, to whom, though he disliked them, he listened, and the advance of age.

His first treatment, was, doubtless, coarse, rude, reckless, and indiscriminate, conducted without knowledge of the human frame and its necessities. Latterly, perhaps, it was in a slight degree less harsh and rude; but its original evils of recklessness and indiscriminate application it retained in good perfection. A peasant, without education, and filled with prejudices, he took no pains either to cultivate his mind or to divest himself of the errors arising from ignorance and prejudice. It may be stated, as a general principle, that all persons in this condition are bold and reckless where they ought to be cautious, and timid, and apprehensive, approaching to superstition, where they might be confident. So was it with Priessnitz originally and his method of treatment. Latterly, we have understood, the number of mishaps rendered him much more cautious. For these, nevertheless, the man was not so much to be blamed as those persons whose daring folly placed themselves under his care. Many patients, whose cases evidently must have brought discredit on the Graefenberg practice, he of late years declined to treat. But the numbers who expected to derive benefit from his management, often prevented him from exercising even this negative sort of selection.

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